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THE PSYCHOLOGICAL REVIEW.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF IOWA.

ON THE EFFECTS OF LOSS OF SLEEP.[1]

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The object of the following experiments was to determine some of the physiological and mental effects of enforced abstinence from sleep. In an address before the International Medical Congress at Rome in 1894, M. de Manacéine reported some experiments upon young dogs on the effects of absolute insomnia. The animals were kept from sleeping, and died at the end of the fourth or fifth day. (*Arch. Ital. Biol.* XXI, 2. PSYCHOLOGICAL REVIEW II, 1, p. 81.) So far as is known to the present writers, no experiments upon human subjects have hitherto been made on enforced insomnia for psychological purposes. The plan of our experiments was as follows: It was proposed to keep the subjects awake continuously for about 90 hours, to make a series of physiological and psychological tests upon them at intervals of 6 hours in respect to reaction-time, discrimination-time, motor ability, memory, attention, etc.; to observe secondly, the general effects of insomnia, and finally to observe the depth, character and amount of sleep following the period of waking. This plan was successfully carried out with three subjects, the depth of sleep being ascertained, however, in the case of only one. The subjects were in each case constantly attended by either one or two watchers.

[1] One of the three experiments described in this article was reported in a paper by Professor Patrick at the December meeting of the American Psychological Association at Philadelphia.

They took their regular meals at 7 a. m., 12.30 p. m., and 6 p. m., the food being normal in character and amount. In addition they ate a very light lunch at 12.30 a. m. The days were spent in occupations conforming as nearly as possible to the usual daily work of the subject. The nights were spent at first in reading or playing light games, and toward the end of the experiments in any way best adapted to keep the subjects awake, such as walking, working upon apparatus, or playing active games. Each set of experiments, however, took nearly two hours, so that this occupation consumed almost one-third of the time both day and night.

We give first a general account of the subjects and experiments. The first subject, J. A. G., is a young man of 28 years, assistant professor in the University. He is unmarried, of perfect health, of nervous temperament, of very great vitality and activity. He is accustomed to about 8 hours of sound sleep from 10 p. m. to 6 a. m. He awoke at his usual time Wednesday morning, November 27, and remained awake until 12 o'clock Saturday night. The second night he did not feel well and suffered severely from sleepiness. The third night he suffered less. The fourth day and the evening following he felt well and was able to pass his time in his usual occupations. During the last 50 hours, however, he had to be watched closely, and could not be allowed to sit down unoccupied, as he showed a tendency to fall asleep immediately, his own will to keep awake being of no avail. The daily rhythm was well marked. During the afternoon and evening the subject was less troubled with sleepiness. The sleepy period was from midnight until noon, of which the worst part was about dawn.

The most marked effect of the abstinence from sleep with this subject was the presence of hallucinations of sight. These were persistent after the second night. The subject complained that the floor was covered with a greasy-looking, molecular layer of rapidly moving or oscillating particles. Often this layer was a foot above the floor and parallel with it and caused the subject trouble in walking, as he would try to step up on it. Later the air was full of these dancing particles which developed into swarms of little bodies like gnats, but colored red.

purple, or black. The subject would climb upon a chair to brush them from about the gas jet or stealthily try to touch an imaginary fly on the table with his finger. These phenomena did not move with movements of the eye and appeared to be true hallucinations, centrally caused, but due no doubt to the long and unusual strain put upon the eyes. Meanwhile the subject's sharpness of vision was not impaired. At no other time has he had hallucinations of sight and they entirely disappeared after sleep.

The period of 90 hours being completed at 12 o'clock Saturday night, the subject was allowed to go to sleep, which he did immediately. He was awakened at intervals of one hour to ascertain the depth of sleep, but fell asleep again at once after each awakening, and slept until half past ten Sunday morning. He awoke then spontaneously, wholly refreshed, felt quite as well as ever, and did not feel sleepy the following evening. He slept, however, two hours later than usual Monday morning.

The special tests made upon this subject, 14 in number, are shown with the results in Table I. They were all repeated every 6 hours throughout the whole period, and repeated again finally after the subject had slept. The results of the latter tests are shown in the last column. In reaction-time and discrimination-time, the effects of practice were eliminated as far as possible by preparatory training preliminary to the experiment. A few words of explanation of methods and apparatus are necessary. The pulse was taken at the beginning of each set of tests and then again at the end immediately after the subject was fatigued by tapping with the forefinger as rapidly as possible for 60 seconds. The subject was weighed the same time after each meal and in the same clothing. Grip was taken with an ordinary hand dynamometer. Pull was taken with the same instrument, the subject using the second finger of each hand.

For reaction-time the stimulus was a telephone click, with signal, the reaction being the release of a key, the subject being in the dark room, away from the recording drum. Each reaction-time given represents the mean value of from 10 to 15 reactions. For discrimination a modification of the same apparatus was used, the subject reacting only to the loud stimulus.

J. A. G.

TABLE I.

		November 27.			November 28.				November 29.				November 30.				Dec. 1. After Sleep.
		9 a.m.	3 p.m.	9 p.m.	3 a.m.	9 a.m.	3 p.m.	9 p.m.	3 a.m.	9 a.m.	3 p.m.	9 p.m.	3 a.m.	9 a.m.	3 p.m.	9 p.m.	12 m.
1. Pulse		88	89	68	62	81	72	74	74	68	65	63	63	61	72	61	77
2. Temperature (Centigrade)		36.72	36.39	36.17	35.78	36.56	36.67	36.56	35.67	36.44	36.56	36.11	36.28	36.00	36.50	36.39	36.17
3. Weight (Kilograms)		67.70	67.75	68.30	67.78	68.19	68.04	68.52	68.83	68.27	67.99	68.35	68.60	68.41	68.13	68.47	67.39
4. Grip (Kilograms)		48.08	46.95	51.94	47.86	47.17	44.45	40.83		44.91	48.08	47.17	45.36	43.99	49.67	43.77	50.35
5. Pull (Kilograms)		27.22	27.67	28.12	26.31	26.76	25.86	22.68	22.68	26.31	25.86	24.95	23.59	22.68	26.99	23.13	27.67
6. Reaction-time.	Mean (Sec.)	.122	.132	.129	.149	.133	.129	.139	.143	.146	.130	.144	.146	.139	.165	.148	.128
	Mean Variation.	9	26	28	20	10	16	24	25	21	10	50	21	31	26	20	13
7. Discrimination-time.	Mean (Sec.)	.258	.240	.242	.253	.225	.215	.216	.271	.207	.210	.213	.213	.206	.201	.158	.205
	Mean Variation	50	56	51	48	38	32	43	67	63	63	40	65	62	36	43	52
8. Sensibility to pain.	Lower threshold		3250	3000	3100	2750	3100	2650	2800	3150	2800	2750	2850	3300	3150	3250	3200
	Upper threshold		4450	4350	4550	4050	4650	4450	4300	4600	4250	4200	4300	4550	4850	4400	4800
9. Acuteness of Vision (cm.)		137.2	132.1	139.7	134.6	142.2	156.2	150.5	120.6	137.2	143.5	137.2	152.4	148.6	156.8	171.4	125.7
10. Memory (Sec.)			540	260	159	290	330	200	105	240	70	262	290	123	190	545	125
11. Addition of Figures			228	254	248	238	249	223	215	205	216	196	210	200	250	224	277
12. Voluntary Motor Ability		42.2	42.2	40.1	39.0	40.0	41.2	38.6	35.5	39.5	39.0	35.0	38.9	41.0	39.0	39.7	41.3
13. Fatigue. Per cent. of Loss		24.1	24.6	22.6	20.5	18.0	24.0	13.7	12.1	17.0	13.9	11.4	20.6	17.6	17.9	13.6	17.7
14. Pulse after Fatigue		89	81	92	82	75	76	58	59	62	62	54	58	63	59	52	84

Sensibility to pain was tested by a specially prepared algometer, arranged to bring any desired pressure upon the middle of the fingernail of the first finger, the finger being inserted between two horizontal bars, the one pressing upon the fingernail being a very dull wooden knife edge. The figures record the pressure in grams, the lower threshold representing the first feeling of pain, the upper threshold the point at which the pain could no longer be endured. Acuteness of vision was tested in the dark room by finding the greatest distance at which the subject could read a section of a page from Wundt's *Studien* by the light of one standard candle at a distance of 25 cm. The memory test consisted in committing to memory 10 of the Ebbinghaus nonsense syllables. These were used in the ordinary way, but we consider this test of very slight value, for it is impossible not to learn these lists by association, and impossible to get different lists which offer equal ease or difficulty in association. The effects of loss of sleep upon attention and association we attempted also to ascertain by determining the greatest number of figures in prepared columns that could be added in three minutes. Voluntary motor ability was tested by having the subject tap with the forefinger as rapidly as possible upon a key for 5 seconds, using the recording drum and graphic chronometer. He then continued tapping for 60 seconds to fatigue the muscles. The number of taps during the last 5 seconds was then recorded. In the table is given first the number of taps in the first 5 seconds, then the percentage of loss in the last 5 seconds due to fatigue. The results of the special tests may best be studied from the table. Attention is called, however, especially to the following. The steady increase in the subject's weight during the experiment and the sudden decrease in weight after sleep are noteworthy, and apparently not to be accounted for by accidental circumstances. His average weight during the last 24 hours was 18 ounces greater than the average during the first 24 hours, and at 9 o'clock Saturday night the subject weighed 27 ounces more than at 9 o'clock Wednesday morning. During the 10½ hours' sleep, however, which followed the experiment, the subject lost 38 ounces, being 11 ounces more than he had gained during the

experiment. In the tests with the dynamometer the subject lost slightly and gradually in strength of both grip and pull, regaining all after sleep. On Saturday afternoon, however, the subject made what appeared to be a spurt, in view, perhaps, of the approaching end, and gripped and pulled nearly as much as at the beginning. The reaction-time beginning with 122σ increased somewhat regularly, reaching its maximum, 165σ Saturday afternoon, after 81 hours without sleep, and dropped back to the normal immediately after sleep. The discrimination-time appears to decrease, but as it does not increase after sleep the result cannot in this case be attributed to loss of sleep. The acuteness of vision uniformly *increased* throughout the experiment, falling below the normal after sleep. The slight retardation in the increase in the second night corresponds with the period of slight sickness at that time. There is a significant decrease in voluntary motor ability. The decrease in this subject's pulse-beat after fatigue by tapping is abnormal and apparently a result of loss of sleep.

The above experiment upon J. A. G. was regarded as somewhat preliminary. It was, therefore, decided to repeat the experiment upon two other subjects, making such modifications in the special tests and apparatus as seemed to be desirable. The second subject, A. G. S., was a young man of 27 years, instructor in the University, unmarried, quiet and of excellent health. The third subject, G. N. B., was a young man of 24 years, instructor in the University, unmarried, of German parentage, stout and perfectly healthy. At the time of the experiment, A. G. S. was accustomed to 9 hours of sound and regular sleep; G. N. B. to 8 hours. These two subjects entered upon their sleep fast at 7 o'clock, Tuesday morning, March 17, 1896. 90 hours was again the period determined upon. On Friday night, March 20, at 11.15, the last set of experiments being completed, they were allowed to retire, so that their waking period was actually 88¼ hours. In the case of these two subjects there was no illness, no hallucinations of sight, and no serious suffering or discomfort. A. G. S. became very sleepy during the last 24 hours and had to be watched constantly. On Friday, at 9 p. m., after a brisk walk in

the cool air, his temperature sank to 35.3° Cent. (95.6° F.), but in 15 minutes rose to 36.3° Cent. (97.3° F.). Of the three subjects he was the only one who apparently could not have prolonged the experiment beyond the period of 90 hours without danger. G. N. B. had less trouble in keeping awake and showed outwardly but slight effects of the abstinence from sleep. Both subjects slept immediately upon retiring at 11.15 p. m., Friday. They both slept uninterruptedly until 10.30 a. m., Saturday. They both awoke then for a few moments and slept again, A. G. S. until 11.15 a. m., G. N. B. until 2.40 p. m. They both felt wholly refreshed upon awaking, required no further extra sleep, and felt no ill effects from the experiment.

The special tests made upon these two subjects are shown with the results in Table II. and Table III., and exhibited, in part, in graphic form in the subjoined curves. They were as before, repeated every 6 hours. To eliminate, as far as possible, the effects of practice, the tests were begun two or three days before the beginning of the sleep fast. The first three sets of results in the tables, being taken the first day before any loss of sleep, should represent the normal reaction of the subject. These, taken together with the results of the tests made after awaking shown in the last column of the tables, make a fairly adequate standard for comparison with the results obtained during the sleep fast. The tests in respect to pulse, temperature, weight, grip, reaction-time, discrimination-time, sharpness of vision, voluntary motor ability, and fatigue, were the same as described above for the first subject. The strength of pull was taken with an ordinary lift dynamometer, the subject, standing upon a small platform with bent knees and straightened back, lifting his utmost by means of two handles connected by ropes with a large spring balance. In the memory test, the nonsense syllables were discarded and 18 figures substituted. 18 small squares of cardboard were provided upon which were printed the 9 figures, each figure thus appearing twice. For each experiment a random order of these figures was made, and then modified, if necessary, to prevent adjacence of same figure and suggestive combinations. The subject, timed with a stop

watch, committed to memory the list, the watch being stopped when the subject announced his readiness to recite the list. Each experiment consisted in committing to memory three such lists. The tables show in seconds the average of these three trials in each case. No. 11 was a test in adding numbers. The sheets of figures used by Miss Holmes in studying fatigue in school children and described in the Pedagogical Seminary, Vol. III., No. 2, were used. The subject was required to add each set of 40 figures by twos, setting down the results. He then added the results and then added the original figures in a different order. Any variation recorded in the two results indicated errors. The tables give the time required for the whole process. Test No. 12 was designed to determine the subject's facility in seeing and naming letters. A page from THE PSYCHOLOGICAL REVIEW was used; the subject reading the lines backward merely named the letters as fast as possible. The tables record the number of letters, average of two trials, named in one minute. Test No. 9 was designed to show the acuteness of hearing by discrimination of the intensity of two sounds. The sounds were vibrations of a tuning fork heard in a telephone in the silent room, the intensity being varied by a resistance board, only one telephone being used. The results in the tables have only relative value, indicating the number of divisions upon the resistance board by which the resistance had to be increased to enable the subject to detect the difference in the intensity of the sounds.

We may call special attention to a few of the results. In both subjects we again observe an increase in weight throughout the experiment with decrease after sleep. But with these subjects the decrease is less than the increase. In strength of lift both subjects lose quite regularly and seriously, but regain nearly all after sleep. In the memory tests, the results are very marked, especially with G. N. B. His average time in normal condition for committing the 18 figures was 134 seconds. No remarkable increase in this time was observed until the expiration of 72 hours. At 9 a. m. Friday the subject required 960 seconds to commit the first set of figures and failed entirely to commit the third set, working at it for *20 minutes*. At 9

A. G. S.

TABLE II.

	March 17.			March 18.				March 19.				March 20.				Mar. 21. After Sleep.
	9 a. m.	3 p.m.	9 p.m.	3 a.m	9 a.m.	3. p.m	9 p.m.	3 a.m.	9 a.m.	3 p.m.	9 p.m.	3 a.m.	9 a.m.	3 p.m.	9 p.m.	1 p. m.
1. Pulse	74	68	75	61	73	73	72	71	79	62	67	61	74	68	63	76
2. Temperature (Centigrade)	37.11	36.39	36.78	37.11	37.00	37.22	36.89		36.89	36.44	36 56	36.33	37.06	36.67	35.33	37.22
3. Weight (Kilograms) . . .	67.02	67.47	67.47	67.24	66.68	67.24	67.13	66.68	67.02	67.36	67.47	67.59	67.02	67.36	67.59	67.24
4. Grip (Kilograms) . . .	33.56	39.92	30.39	33.11	33.56	29.03	24.04	24.04	28.12	29.48	26.31	26.76	29.03	30.39	27.22	33.56
5. Pull (Kilograms) . .	155.58	163.30	140.62	117.94	150.60	113.40	127.00	81.65	107.05	89.36	88.45	49.44	49.44	95.26	92.99	131.54
6. Reaction-time. Mean	.121	.134	.138	.134	.141	.138	.143	.154	.147	.150	.141	.146	.143	.148	.193	.160
Mean Variation	0.6	1.5	0.8	0.9	2.2	1.2	7.1	1.5	1.7	1.9	2.4	1.5	1.9	2.5	4.0	2.9
7. Reaction-time with discrimination and choice. Mean	.158	.200	.310	.175	.202	.201	.182	.162	.188	.280	.189	.170	.222	.176	.311	.231
Mean Var.	5.9	4.2	7.4	4.1	3.5	4.5	2.9	3.6	4.1	8.3	4.7	3.9	5.3	4.3	8.0	6.0
8. Acuteness of Vision (C.M.)		103.8	103.8	113.6	122.3	112.8	96.1	105.1	115.4	116.6	119.2	109.0	119.7	118.0	123.3	119.7
9. Discrimination of Sound .	8.0	12.5	10.0	11.2	11.6	13.0	12.5	31.0	12.5	22.0	21.0	31.0	23.0	18.7	18.0	16.5
10. Memory (Sec.)	115	110	112	143	129	145	102	159	120	152	217	202	139	100	570	88
11. Addition of Figures . . .		85	119	118	105	103	130	192	111	108	185	610	113	190	345	109
12. Naming of Letters	165	160	155	154	162	155	134	113	154	144	127	91	147	135	117	171
13. Voluntary Motor Ability .	38	36	33	37	41	36	30	34	36	36	37	28	39	38	34	42
14. Fatigue. Per cent. of Loss	29.0	13.9	15.1	13.5	29.3	16.6	13.3	26.5	19.4	25.0	21.7	0.00	20.5	23.7	26.5	21.4
15. Pulse after Fatigue . . .	80	69	69	66	79	71	77	65	72	75	64	62	70	64	61	83

G. N. B.

Table III.

	March 17.			March 18.				March 19.				March 20.				Mar. 21. After Sleep.
	9 a.m.	3 p.m.	9 p.m.	3 a.m.	9 a.m.	3 p.m.	9 p.m.	3 a.m.	9 a.m.	3 p.m.	9 p.m.	3 a.m.	9 a.m.	3 p.m.	9 p.m.	4 p.m.
1. Pulse	63	64	63	68	68	67	67	69	70	62	64	68	74	65	73	84
2. Temperature (Centigrade)	36.22	36.44	36.33	37.17	36.78	37.22	36.56	36.67	36.33	36.61	36.56	36.89	37.06	35.78	36.56	37.22
3. Weight (Kilograms)	68.49	69.29	69.29	69.17	69.51	69.74	69.85	69.99	69.85	69.99	70.08	70.08	69.40	69.74	69.85	69.29
4. Grip (Kilograms)	42.64	34.47	38.10	33.11	39.36	43.09	37.65	34.01	37.19	37.65	42.64	43.09	44.45	47.63	44.00	41.73
5. Pull (Kilograms)	118.84	129.28	146.15	138.35	125.19	117.94	106.60	111.13	120.20	113.40	113.40	113.40	113.40	111.13	95.26	117.94
6. Reaction-time. Mean	.145	.148	.157	.130	.142	.143	.134	.187	.136	.137	.141	.123	.139	.141	.142	.124
Mean Variation	1.8	1.3	1.4	0.8	1.1	1.7	1.8	2.9	3.7	1.0	1.7	1.8	1.1	2.9	3.2	1.6
7. Reaction-time with discrimination and choice. Mean	.167	.170	.200	.140	.185	.177	.214	.170	.178	.147	.158	.133	.153	.143	.175	.166
Mean Var.	3.6	7.2	5.6	1.4	3.7	1.6	5.5	4.6	6.8	1.8	2.7	3.7	2.3	1.2	2.3	4.0
8. Acuteness of Vision		110.3	115.4	141.0	132.1	134.6	127.4	129.5	134.6	119.2	137.2	126.9	130.9	128.7	135.9	134.6
9. Discrimination of Sound	12.8	20.0	12.5	24.8	14.0	15.0	20.5	17.5	24.5	30.0	23.0	20.0	22.5	21.0	32.0	21.5
10. Memory	170	133	128	206	170	135	273	143	112	353	169	201	820+	645	900+	106
11. Addition of Figures		120	125	141	135	122	140	141	135	120	118	115	118	123	130	109
12. Naming of Letters	177	180	169	165	183	163	158	158	156	165	154	156	157	148	117	188
13. Voluntary Motor Ability	41	37	38	39	41	42	34	39	39	39	40	44	42	42	40	40
14. Fatigue. Per cent. of Loss	19.5	16.2	26.3	28.2	29.3	28.6	14.7	28.2	25.6	33.3	25.0	34.1	26.2	26.2	35.0	22.5
15. Pulse after Fatigue	70	69	60	69	79	63	64	69	79	64	55	64	77	69	70	96

p. m. he could not commit the figures, and having made no progress after 15 minutes he desisted. The attention could not be held upon the work. A kind of mental lapse would constantly undo the work done. With both subjects an energetic 'waking up' by means of brisk walking and fresh air was often necessary during the latter time in order to address themselves to these mental tasks. After sleep, A. G. S. easily committed the figures in 88 seconds, and G. N. B. in 106 seconds, this being in both cases the shortest time in which the work was done. In respect to the number of letters named in one minute, there is with both subjects a steady decrease with the progress of the insomnia, with immediate return to the normal after sleep. In adding numbers similar results appear in a marked form in the case of A. G. S., but with G. N. B. adding time was affected but slightly. Reaction-time increases with A. G. S., as with J. A. G., but the reaction-time of G. N. B. is not lengthened. In respect to reaction with discrimination and choice the results are irregular and unsatisfactory. There is an irregular increase with A. G. S., but an actual shortening of time with the other two subjects.

Attention should be called to the length of sleep following the sleep fast and its relation to the whole amount of sleep lost. A. G. S. found it necessary to make up but 16 % of the lost sleep, as measured by time; J. A. G. 25 %; G. N. B. 35.3 %; As restoration was in each case apparently complete, explanation must be sought in one of two hypotheses or in both. The first is that, owing to the greater 'depth' of sleep after the sleep fast, the anabolism accompanying restoration was more rapid. The second is that the partial restoration which normally accompanies the waking period was, in the case of this long waking, greater than usual; that the subjects, in other words, although apparently awake and, indeed, as wide awake as they could be kept, were nevertheless at times partially asleep. There are reasons to believe that the results depend upon both of these causes. Our subjects well illustrated the fact that sleep is a matter of degree. All that could be done both by objective diligence and subjective effort to keep the subjects wide awake was done. If the subject, contrary to his own intention, closed

his eyes, although he immediately opened them in response to his watcher's command, still there was time for a short and, perhaps, refreshing 'nap.' Again, one of our subjects, who was kept jogging about the streets during a sleepy period at 5 a. m., afterwards could remember little about the walk. Another subject, standing with eyes open, reflectively gazing at a piece of apparatus upon which there were some pieces of rope, suddenly reported that he had had a dream about a man being hung. With our first subject we undertook to test the delicacy of the muscle sense by means of lifting weights. These weights were small tin pails loaded with graded weights and lifted by a detachable handle. Lifting these pails was found to be very monotonous and sleepy work. The subject was not permitted to let his attention wander, and yet he reported at least four dreams. For instance, he lifted two pails, carefully judged their relative weight, and as he set the second one down, instead of saying that No. 1 or No. 2 was the heavier, he said 'trimmings,' evidently having fallen asleep as he was lifting or setting down the pails and dreamed that they contained trimmings. It must be understood that these dreams were instantaneous and the subject as wide awake as he could be kept, but these facts reveal a cerebral condition related to sleep. This hypothesis alone, however, would not seem to account fully for the small proportion of sleep made up. And, indeed, a study of our special tests shows that restoration took place chiefly during the profound sleep following the sleep fast, and took place rapidly. That this sleep was actually more profound and that the profound part of it was longer than usual was shown by our experiments in depth of sleep in the case of J. A. G. reported below.

The depth of normal sleep for the consecutive hours of the night has been studied by Michelsen and by Kohlschütter, and the results presented in the so-called sleep curves. The depth of sleep was determined by these observers by the intensity of sound necessary to awaken the sleeper. Their results show the greatest depth of sleep at the end of the first hour. After the first hour the curve drops abruptly and rapidly. Already at the end of the second hour sleep is light and continues slowly

to become lighter until morning. In the case of our first subject, J. A. G., we attempted to ascertain the relative depth of sleep for the consecutive hours of the profound sleep following the sleep fast, for the sake of comparing our results with the normal sleep curve. As a sound stimulus would not be practicable, for the reason that, the experiments all being made in the same period of sleep the sleeper would soon become accustomed to it, we substituted a pain stimulus. An electric garter, to which the subject had become accustomed by wearing it for some nights preceding the sleep fast, was attached to the sleeper's ankle and connected with an induction coil in an adjoining room, and so arranged that the current could be closed for a constant time, viz., .334 sec., by means of a pendulum, and that the strength of the current could be varied by means of a resistance tube. It was agreed that the sleeper should announce his awaking by means of an electric button at his bedside. The current was turned on at intervals of one hour. Unfortunately the least resistance that could be arranged with the resistance tube failed to awaken the sleeper at the first three periods, so that it was necessary to cut out the tube and the pendulum and apply the direct current and measure it roughly by the time the circuit had to be closed. Our results, therefore, lack the exactness necessary for the construction of a curve or table, but still show plainly the relative depth of sleep for the consecutive hours. The deepest sleep was found at the end of the second hour, when the subject could not be aroused sufficiently to ring the bell, but responded by a cry of pain. The next deepest sleep was found at the end of the first hour and the next at the third hour. The current used at these three times was one which it was altogether out of the question for the subject to endure when awake. At the end of the second hour, just after the experiment, we entered the sleeper's room and attempted to awaken him by speaking to him in a loud voice without avail. At the fourth hour the sleep was less deep, and continued to become lighter regularly until awaking, but the decrease in depth was very much less rapid than in the normal sleep curves reported above. At 10 a. m. a very slight current awakened the sleeper, and at 10:30 he awoke spontaneously as stated.

The tendency of our subjects to have short semi-waking dreams suggested to us that in enforced insomnia there would be offered a good opportunity for a study of dreams. This, of course, was incompatible with our purpose, but in the cases of A. G. S. and G. N. B., at the end of the sleep fast and before allowing the subjects to retire, we undertook a few experiments in dreams. We allowed the subjects to sit with head supported behind, and to sleep for periods of 30 seconds, one

TABLE IV.

J. A. G.	2d day before experiment.	1st day before experiment.	1st day of experiment.	2d day of experiment.	3d day of experiment.	4th day of experiment.	4th day of experiment. (Sleep.)	1st day after experiment.	2d day after experiment.
Hours			24	24	24	14	11¾	24	
Total amount urine (ccm.)			1475	1370	1270	805	400	950	
Grams N. per hour . . .			0.901	0.929	0.667	0.723	0.490	0.723	
Grams P_2O_5 per hour . .			0.1327	0.1438	0.1105	0.1304	0.0564	0.0888	
Relation P_2O_5 to N. . . .			1: 6.8	1: 6.5	1: 6.0	1: 5.5	1: 8.7	1: 8.1	
A. G. S.									
Hours	38		24	24	24	13½	12¾	24	24
Total amount urine (ccm.)	1308		1510	1700	1420	750	525	1000	1240
Grams N. per hour . . .	0.655		0.661	0.628	0.745	0.661	0.414	0.6175	0.761
Grams P_2O_5 per hour . .	0.0765		0.0708	0.0791	0.1011	0.1000	0.0674	0.0907	0.1023
Relation P_2O_5 to N. . . .	1: 8.6		1: 9.3	1: 7.9	1: 7.4	1: 6.6	1: 6.1	1: 6.8	1: 7.5
G. N. B.									
Hours	24½		24	24	23	13½	16½	24½	24
Total amount urine (ccm.)	920		1240	1205	1730	650	365	705	705
Grams N. per hour . . .	0.4853		0.7094	0.6270	0.6123	0.5195	0.3390	0.5020	0.4765
Grams P_2O_5 per hour . .	0.0574		0.0802	0.0931	0.0826	0.0815	0.0435	0.0616	0.0613
Relation P_2O_5 to N. . . .	1: 8.5		1: 8.8	1: 6.7	1: 7.4	1: 6.4	1: 7.8	1: 8.1	1: 7.8

minute, three minutes, etc., then awakening them and asking for their dreams. No dreams were obtained in any case. If the period was less than one minute the subject sometimes had a hazy memory of something like a dream which could not be put into words. If the sleep was longer it was apparently profound and dreamless. These rough experiments confirm, of course, the generally accepted opinion that dreams are the product of light sleep, representing indeed the reinstatement of consciousness after the early and profound sleep.

Through the kindness of Dr. E. W. Rockwood, of the University, a chemical analysis of the urine was made throughout the experiments in the case of each of the subjects. The object of the analysis was to determine the influence of continued waking upon the relative amounts of nitrogen and phosphoric acid respectively excreted. The results are fully exhibited in Table IV. as compiled by Dr. Rockwood. Considered in relation to the fact that each subject increased in weight during the insomnia, the results are significant. They show not merely that there was an increase in the excretion of both nitrogen and phosphoric acid during the period of insomnia, but that relatively more phosphoric acid was excreted than nitrogen. A certain amount of support is thus given to the theory of a special connection between mental activity and the katabolism of the phosphorized bodies of the nervous system.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF HARVARD UNIVERSITY.

I. The Relations of Intensity to Duration of Stimulation in our Sensations of Light.

BY JAMES E. LOUGH.

Instructor in Psychology, Harvard University.

These experiments were made for the purpose of ascertaining the exact relation existing between the duration of a stimulus and the intensity of the resulting sensation. They were suggested by the phenomena of color-mixing by means of Maxwell's discs. This method of color-mixing shows that the influence of a given color upon the final mixture varies with the size of the sector of that color. In these rotations the color does not vary in intensity, but the time during which it stimulates a given portion of the retina changes. The relative time, however, is the only effective element, for after the colors once fuse, any increase in the speed of a rotation produces no change in the intensity of the colors.

My first experiments repeated the conditions given by Maxwell's discs, but with apparatus so arranged that the experimenter could determine the exact amount of variation in the intensity of the sensation, resulting from a given difference in the duration of the stimulating light.

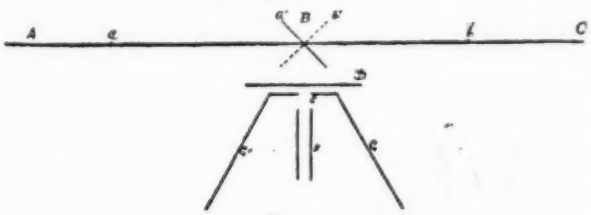

Fig. 1.

The apparatus used may be understood from the ground plan shown in Fig. 1.

A B and B C are two wooden arms along which slide lamps *a b* of one standard candle power each. The lamp *a* stands a little higher than *b*; and the dead white reflector *a'*, standing at its level, reflects its light through the upper half of the slit E. The reflector *b'* reflects the light from *b* through the lower half of the same slit. G is a large black screen to protect the subject's eyes from lateral lights. In it is the slit E 1 cm. wide by 4 cm. high. F is a black tube to fix the eye and still farther cut off side light. D is a dead black disc rotated by a color wheel. A window, *d' d''*, is cut out of this disc, as is shown in Fig. 2.

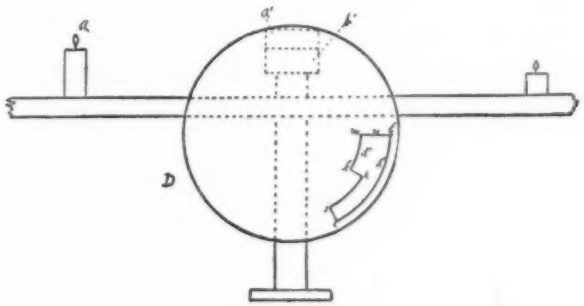

FIG. 2.

The lines *m h*, *l k* and *j i* are radial and *m l*, *k j* and *h i* arcs of concentric circles. D is placed so that when the window covers the reflectors *a'* and *b'* the line *j k* is level with the horizontal line dividing them. Consequently *a'* stimulates the eye while *d'* is passing between it and the slit, and *b'* while *d''* is passing. The absolute duration of this stimulation will depend upon the rotation-rate of the disc, but we have seen that it is only the relative time we need to consider in comparing the effects of stimuli of different duration. The relative times of exposure to the eye of the lamps *a* and *b* will be proportional to *i n* and *k n* severally.

The room was darkened and the lamps placed 20 cm. from the reflectors (correction being made in this and every other experiment reported for any over-estimation of intensity due to position, etc.). The disk, with j^n and k^m in the ratio of 2 : 1, was rotated 100 times per second, under which conditions the

after-images from a' and b' fused completely, so that each reflector gave a continuous impression. But the lower one now appeared much darker than the upper one. In order to determine how much darker, the lamp b was moved toward b', thus increasing its objective intensity until the two reflectors again appeared equally illuminated. In other words, until the intensity of sensation lost through the shorter time of stimulation was compensated by the greater intensity of the stimulus. The relative intensity of the reflected lights may now be calculated from the distances of the lamps, and the ratio between the original intensity of b and the final one will express the loss of intensity due to its shorter time of stimulation.

The results of this experiment are given in Table I., each ratio given being the average of ten determinations. The subjects were Dr. Singer and the writer.

TABLE I.

Ratio of $d' : d''$	Ratio of Intensity.	Subject.
1.35 : 1	1 : 1.35	S.
1.35 : 1	1 : 1.39	L.
2.05 : 1	1 : 2.05	L.
2.93 : 1	1 : 2.97	S.
3.00 : 1	1 : 3.02	L.

It is clear from this table that when the difference of time between d' and d'' is not greater than that here employed, *the intensity of the resultant sensation is proportional to the time of stimulation.*

A second series of experiments followed these, differing from them only in this, that the light from a' was not interrupted at all, and hence always produced its maximum effect. The light from b' was interrupted by sectors of the disc D. If S represents the width, in degrees, of the sector, then 360 : 360–S will represent the relative duration of the stimuli from a' and b'. The rapid rotation of the disc caused b' immediately to appear much darker, a', of course, remaining unchanged. The intensity of b' was then made to equal a' by moving the lamp b nearer its reflector. From these data it is

possible to determine the loss of intensity due to the shortened time. The experiment was made with light of different intensities. That reported in Table II. was produced by one candle-power at 100 cm. The experiments using other intensities gave the same results. Each number in the table represents the average of a large number of determinations. In this and all the following experiments the writer was the only subject, other persons being used only to confirm the results given.

TABLE II.

Ratio of duration.	Ratio of intensity.	Ratio of duration.	Ratio of intensity.
1 : .0055	1 : .006	1 : .222	1 : .239
1 : .0083	1 : .0078	1 : .25	1 : .272
1 : .011	1 : .011	1 : .306	1 : .385
1 : .014	1 : .012	1 : .361	1 : .463
1 : .0166	1 : .0164	1 : .5	1 : .538
1 : .0194	1 : .0177	1 : .611	1 : .645
1 : .0222	1 : .0225	1 : .666	1 : .662
1 : .025	1 : .0249	1 : .702	1 : .7
1 : .0277	1 : .0273	1 : .705	1 : .702
1 : .0555	1 : .059	1 : .803	1 : .813
1 : .083	1 : .091	1 : .888	1 : .909
1 : .1111	1 : .121	1 : .902	1 : .943
1 : .139	1 : .156	1 : .904	1 : .97
1 : .166	1 : .176	1 : .97	1 : .98
1 : .194	1 : .209		

This table shows that throughout the entire series a decrease in the time of stimulation results in a proportional decrease in the intensity of the resultant sensation. All of these experiments were then repeated with colored lights, produced by interposing gelatine sheets; red, green, blue and yellow were used. These gave the same results as those given in Table II.

It would appear from these experiments that the chemical processes in the retina take place only after a certain inertia has been overcome, and that this requires a certain duration of stimulation. When under ordinary conditions a stimulus of a given intensity excites the retina it produces a chemical dis-

integration, which is a growing process up to a fixed limit, which depends upon the intensity of the stimulus. When this limit is reached, the light produces its maximum effect. Beyond the point of maximum effect, time produces no difference of intensity; looking at a lamp for two minutes does not make it seem brighter than when it is seen for only one minute. But below this point of maximum effect, the duration of the stimulus is one of the factors determining the amount of disintegration in the retina and so the intensity of the resulting sensation. It was the object of a third series of experiments to find the point of maximum effect. The apparatus was the same as that already described, except that a large dead black screen swinging upon a pendulum apparatus took the place of the disc D. The screen contained a window similar to that in the disc.

Let us call the upper and narrower half of this opening s', and this lower and wider half s'', and the reflectors back of the screen a' and b' as before. The opening was so arranged that at the lowest point of the swing, both reflectors came simultaneously into view, a' being seen through s', and b' through s''. The relative time during which a' and b' will stimulate the eye will depend upon the relative width of s' and s'', while the absolute time of both stimulations will depend upon the arc through which the pendulum swings. The absolute time of exposure was determined for each degree of swing by the ordinary means. The pendulum apparatus was made especially for this laboratory by Elbs Freiburg, after Münsterberg. The length of pendulum is 2 meters, but adjustable weights and counterweights give every rate of swing desired. The screen and opening were made in Cambridge, Mass.

In order to obtain the time for maximum effect, the lamps were placed so that a' and b' gave sensations of equal intensity and s' and s'' were then adjusted to the relation of 1 : 2. When now the pendulum was allowed to swing through a small arc the two reflectors seemed equal, but as the amplitude of the swing increased, a point was soon reached where a' appeared just perceptibly darker than b'. This marks the point where a' fails to produce a maximum effect. With the openings and lamps adjusted as before, the pendulum was now given a

much larger swing. This caused a' to appear much less intense, while b' retained its former intensity. The amplitude of the swing was gradually decreased; with this a' becomes gradually more intense until it finally becomes equal to b'; after this no farther change will take place. This also makes the point of maximum effect for a'. These points were ascertained by a large number of experiments and their mean taken as the real time necessary to produce a maximum effect. This point of maximum effect was found for light of several intensities, as given in Table III. The light of a single candle-power lamp at 320 cm.—the limit of the apparatus—was taken as the unit.

TABLE III.

Intensity of Light.	Time of Maximum Effect.
1.	148 σ
4	110 σ
16	100 σ
64	85 σ
256	90 σ

It will be seen from this table that while the time becomes a little longer for the weaker stimili it remains very nearly the same for all but the very lowest. Other subjects gave similar results, but the absolute times varied somewhat.

It should be remarked here that the point of maximum effect —where duration influences intensity—is in no way connected with the threshold for time judgments. The judgment of a difference of duration does not go over into a judgment of difference of intensity.

A third series of experiments were made to determine the amount of intensity lost in a single stimulation by any given reduction of time below that required for the maximum effect. Two methods were employed to reduce the time, giving rise to two sets of experiments. One method used a difference of swing, while the window remained constant; the other varied the size of s', while the swing was the same throughout. The first series employed the method of right and wrong cases to determine the position of the lamps; the second used the method of just perceptible differences.

The first of these experiments was as follows: The pendulum apparatus above described was used, s' and s'' were adjusted in the relation 1 : 2, and the lamp b placed 20 cm. from the reflectors. The pendulum was allowed to swing through a given arc, and a moved until a' appeared similar to b'. Table IV. gives the results of this experiment. The two series were separated by several months, both are given here to show the constancy of the results. Each number is determined by the method of right and wrong cases.

TABLE IV.

Arc.	Duration of stimuli. $S'\sigma$	$S''\sigma$	Ratio of time.	Ratio of intensity. 1st series.	2d series.
60°	30	60	1 : 2	1 : 1.99	
50	40	80	1 : 2	1 : 1.94	
40	60	120	1 : 2	1 : 2.03	1 : 2.03
30	85	170	1 : 2		1 : 1.85
24	110	220	1 : 2	1 : 1.69	
20	150	300	1 : 2	1 : 1.23	1 : 1.23
18	180	360	1 : 2	1 : 1.08	1 : 1.06
16	250	500	1 : 2	1 : 1.10	1 : 1.00
15	310	620	1 : 2	1 : 1.10	1 : 1.02

We see from this table that until 40° is reached the lights keep the same ratio as the openings. Both reflectors did, however, become lighter as the duration of the stimulation became longer. Below 40° b' remains constant and a' approaches it; in other words, b' produces its maximum effect, at between 120σ and 170σ. After 18° the two reflectors are of equal intensity; a' is also producing its maximum effect. This point is somewhere between 150σ and 180σ. These numbers differ from those given in Table III. But the determinations for Table III. were made with a more perfect reflector, giving a much more intense light. Figure 3 gives the curve of intensities as obtained from Table IV.

Between the two points of maximum effect the intensity of the sensation is seen to be exactly proportional to the duration of the stimulus.

The other set of experiments under this head gave a wider

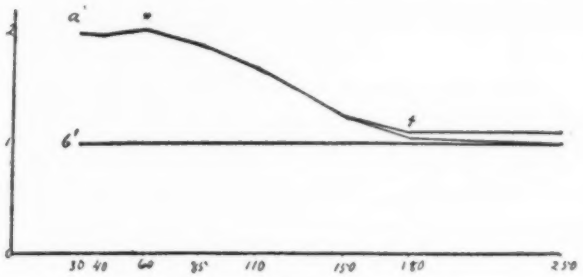

FIG. 3.

range of time for investigation. By the first method the time was limited by the maximum point for b'. With the second method the pendulum swung through a constant arc, and s'' also remained constant, always producing a maximum effect, while the duration of a' was regulated by the size of s'. The intensity of light chosen was one candle-power at 80 cm. The time of maximum effect was first found for s' when s' and s'' were in the rated 1 : 2 in the manner already described. It was found to be 100 σ, and this was taken as the unit of time.

The time of a' was now made one-half of the standard, 100σ, by reducing s', and the loss of intensity resulting determined as before. This was repeated with other lengths of stimulation as given in Table V.

The first column gives the duration of a' in fractions of the maximum time, the second column giving the corresponding reduction of intensity.

TABLE V.

	Time.	Intensity.
1	1.	1.
2	.5	.55
3	.25	.33
4	.125	.115
5	.0625	.065
6	.0312	.029

The exact relation between the duration of short stimuli and the intensity of the sensation must be accepted. We see that it

holds both for rapidly succeeding stimuli and for single stimulations.

The inertia of the retina against chemical disintegration may be accepted as a fact. The amount of this disintegration determines the intensity of the sensation. A strong stimulus acting for half the time necessary to produce its maximum effect gives rise to a sensation of exactly the same intensity as that produced by half as strong a stimulus producing its maximum effect. The stronger sensation does contain the weaker, temporally, for between the first moment of stimulation and the moment of maximum effect the disintegrating process will pass through the series *o* to this maximum. Each step in this series is the basis of a sensation of corresponding intensity. While these growing sensations as such do not enter consciousness, they may be the elements of our feeling of 'more' or 'less,' as concerning the intensity of sensations.

In this way we may conceive of a physiological basis of intensity which does not give a qualitative difference to the sensation.

II. Normal Motor Automatism.

BY LEON M. SOLOMONS AND GERTRUDE STEIN.

It is well known that many hysterical subjects exhibit a remarkable development of the subconscious life, amounting, in many cases, to that most interesting phenomenon known as double personality. It has often been argued that the performances of these 'second personalities' are essentially different from the merely automatic movements of ordinary people—so different, in fact, as to compel us to accept the name 'second personality' as a literal expression of the real state of things. Against this view it is urged that we underestimate the automatic powers of the normal subject. We are told that many of the acts which we usually do quite consciously might really be done without consciousness. In support of this assertion such facts are pointed out, as men completely undressing without knowing it, when their attention is distracted by other matters. If this

latter explanation is to hold, however, something more than assertion must be forthcoming. The limit of automatism is something that is essentially capable of demonstration by experimental methods, and its investigation forms the subject of this paper.

It must not be understood that any attempt is made to answer the vexed question of a so-called 'subliminal consciousness.' This question cannot be settled experimentally, unless it be admitted beforehand that the automatic acts of normal subjects, between which and the 'second personality' an analogy is asserted, are themselves unaccompanied by consciousness. But this is by no means universally admitted. The question of consciousness, in all cases where it is not directly experienced, is essentially a philosophical one, and the facts of psychology have little, comparatively, to do with it. But the question of whether the performances of the 'second personality' are to be allied to the automatic acts of ordinary people, or whether they are to be allied to those acts which never go on save in the full glare of consciousness—by the aid of reflection, judgment and will; this question is perfectly definite, capable of satisfactory solution by observation and experiment, and of great importance to scientific psychology.

The object of our experiments, then, was primarily to determine the limits of normal automatism, and, if possible, show them to be really equal to the explanation of the second personality; and incidentally to study as carefully as possible the process by which a reaction becomes automatic. Above all, we wished to avoid anything like a real production of a second personality. For the experiments to really settle the point at issue it was essential that no suspicion should rest upon the complete 'normality' of the subject throughout the experiments. Our idea was to reproduce rather the essential *elements* of the 'second personality,' if possible, in so far as they consist of definite motor reactions unaccompanied by consciousness—or shall we say, out of deference to the subliminal consciousness theory, unaccompanied by 'conscious consciousness.' These elements appeared to us to be conveniently considered under four groups, as follows:

1. General tendency to movement without conscious motor impulse.

2. Tendency of an idea in the mind to go over into a movement involuntarily and unconsciously.

3. Tendency of a sensory current to pass over into a motor reaction subconsciously.

4. Unconscious exercise of memory and invention.

In the complete second personality all these elements exist at once. We proposed to prove their existence in normal subjects separately.

1. General tendency to movement. For these experiments a planchette was used. Both of us had previously tried in vain to 'write planchette.' Neither of us has any aptitude for willing games, etc. We may both as far as we know stand as representatives of the perfectly normal—or perfectly ordinary—being, so far as hysteria is concerned.

The planchette used was a glass plate mounted on metal balls, with a metal arm holding a pencil. The subject placed one hand firmly on this and then proceeded to get himself as deeply interested in a novel as possible. In this way it is easy to show that although the arm does not really move spontaneously, yet any movement once started up tends to continue of itself. Further, very slight stimuli are capable of starting the movement. For example, as soon as the position of the arm grows uncomfortable, or would be uncomfortable if the subject attended to it, it is likely to begin movement. By slightly moving the planchette it is easy to start the arm to moving, after which it will continue of itself if not deliberately checked by the will of the subject. If the story that the subject is reading be sufficiently interesting, all this goes on without his knowledge. Where he is conscious of the movements of his arm, however, they appear to him to be *extra personal*. It is not he but his arm that is doing it. He cannot say whether his arm is moving spontaneously or whether it is being moved by the operator. Later, if allowed practice, he may learn to make this distinction, but the movements do not at all lose their extra personal character. He readily perceives that they are of two kinds depending on whether the operator moves the planchette or his

arm moves, but both these movements seem equally disconnected with himself. He gains his knowledge of the movement purely through sensations from the arm. He has no feeling of intention or desire; no fore-knowledge of what the movement is to be. As we shall see, this feeling of extra personality appears in all our experiments whenever knowledge of movement is gained purely from sensations—whenever there is no preceding feeling of intention. Where the attention of the subject is completely distracted by the reading, all knowledge of the experiment disappears and the movements go on entirely without his knowledge and quite as well. The only interference comes if the story gets too exciting, when emotional reflexes are likely to interfere either by causing violent movement or by stopping all movement.

Sometimes it is possible to 'teach' the arm some special movement, which it will then go on making of its own accord. For example, the operator may start the planchette to making *m* strokes, and as soon as the hand has caught the movement—shown by the absence of resistance—stop. The arm goes on making the strokes. Gradually, however, it gets them more and more out of shape until it has got into an elliptical movement which is more natural to it, apparently. When this habit —that of making wide elliptical movements, has become well developed, the arm loses its 'suggestiblity' and can no longer be taught special movements. The moment the planchette is released it starts back to its own movement. In connection with this natural movement it should be noticed that it is much more difficult for the subject to distinguish between spontaneous movements and movements impressed by the operator, when the impressed movement is the natural one, than when it is widely different from this. Apparently the arm quickly falls in to the suggested movement when it is its own natural movement; while in other cases this falling in is delayed, resulting in a tension in the muscles of the arm representing its 'hanging back' behind the movement impressed on the hand by the motion of the planchette. It is by learning to recognize this tension that the subject is enabled to distinguish between spontaneous and impressed movements. Introspectively this seemed

to be about the method, that is, and it agrees well with the fact just noted.

From these experiments we concluded that in normal subjects there is a general tendency to movement from purely sensory stimuli, independent of any conscious motor impulse or volition. This tendency is ordinarily inhibited by the will, but comes out as soon as the attention of the subject is removed. This tendency to stop automatic movements and bring them under the control of the will is very strong. Nothing is more difficult than to allow a movement of which we are conscious to go on of itself. The desire to take charge of it is almost irresistible. But as we shall see later it is a habit that can be overcome, and a trained subject can watch his automatic movements without interfering with their complete non-voluntariness.

From now on, having demonstrated the tendency to spontaneous movement, we did not hesitate to make the mere movement element voluntary.

2. Tendency of ideas to go over into movement. For these experiments the subject was given a pencil which he kept moving over a paper as though writing—a sort of continuous movement—he meanwhile being engaged in reading a story. The writing movements quickly become automatic, and nothing prevents the subject from giving his full attention to his reading. Under these circumstances there is a very decided tendency to write down words read, especially simple words such as the, in, it, etc.

Sometimes the writing of the word was completely unconscious, but more often the subject knew what was going on. His knowledge, however, was obtained by sensations *from the arm*. He was conscious that he just *had* written a word, not that he was about to do so. While mere scribbling went on the subject would scarcely be conscious that he was doing anything; but the writing of a word—either because of the different character of the movements, or their greater energy—seemed to attract his attention. Small words would usually be completely written before the subject knew about it, but large words would only get started. But even where there was no interference from the attraction of the voluntary attention large words were

seldom attempted, and, still more rarely, more than just begun. This fact may, however, very easily be referred to the fact that reading is so much faster than writing that subsequent words, with different motor reactions, interfere with the writing of a long word. But a word that can be written with one impulse is not affected by this. Succeeding words may be read before it is written, but their motor impulses do not reach the arm in time to interfere.

As experiments of this kind were of necessity also carried on during the next series, they were not prolonged.

3. Unconscious passage of sensation into motor reaction.

The first form of this which we tried was writing at dictation. As in the other experiments, the subject's attention was occupied as fully as possible in reading. He kept his pencil moving constantly, scribbling when no dictation was going on. These experiments were by far the most difficult we attempted, and required the most training.

At the first attempt the subject is entirely unable to follow what he is reading. He reads, but does not get the meaning. He is painfully conscious of the experiment and everything connected with it. He has an irresistible tendency to stop whenever a word is given to him and attend to that until it is written, and then go on with his reading. In a word, the conditions demanded by the experiment are opposed to all his habits of attention, and the successful carrying out of the experiment demanded that these habits be overcome. And yet, in spite of this, there were momentary lapses of consciousness right from the start. Very uncertain in character and very rare, but enough to encourage us to persevere.

One very quickly gets sufficiently accustomed to the experiment to follow the story. But the habit of turning the attention to the writing whenever a word is given is difficult to overcome. The facility one acquires in rapidly shifting the attention from reading to writing and back, without confusion or effort, is really quite remarkable. Where at first the effort produces nothing but confusion of the worst kind, in a few hours' practice one is able to read his story with perfect ease and comfort, undisturbed by the constant interruptions for writing, even when

these are quite frequent—say every 15 or 20 seconds. But when the story grows interesting the attention is held too powerfully for this, and cases of pure automatism begin to appear frequently. The word is written or half written before the subject knows anything about it, or perhaps he never knows about it. For overcoming this habit of attention we found constant repetition of one word of great value. By such methods as these we gradually began to get control of our attention, and produce the necessary conditions for the experiment. There are four elements to be distinguished in the writing of a word at dictation. 1, The heard sound; 2, the formation of a motor impulse; 3, a feeling of effort; 4, sensation from the arm telling of the written word. 2 and 3 are frequently indistinguishable in consciousness, but they are distinct, for they come and go under different circumstances. 2 consists of a melange of visual and kinæsthetic material—whatever ordinarily innervates our writing—as well as other elements not easily described, and perhaps really a direct consciousness of a motor current. On this point more later.

The first thing to disappear is the feeling of effort. We hear the word, have an idea of how it should be written, and then it is written. The writing seems perfectly voluntary, but there is no sense of difficulty, of 'something accomplished.' The strong self-consciousness that accompanies a concentration of the will at any point is entirely lacking, but nevertheless the writing feels thoroughly voluntary. This feeling of effort reappears after a while, and then it is time to stop the experiment, for the arm is tired. It comes back also if the voice of the operator falls too low.

The next step is the disappearance of the motor impulse. The writing becomes non-voluntary. We hear the word, and we know what we have written; that is all. This is the general condition of things throughout the experiment, after the preliminary training is over. The writing is conscious, but non-voluntary and largely *extra personal.* The feeling that the writing is *our* writing seems to disappear with the motor impulse. This fact is doubly significant here, for in this case we have a fore knowledge of what the written word will be,

since we hear this dictated word. The reaction of the arm is not really unexpected, yet it is still not felt to belong to the willing subject. It sometimes seemed that the visual element of the motor impulse might remain, and the reaction still feel extra personal. But opportunities for observing this were few, and we advance the proposition with hesitation. If true it would lead to the conclusion that the motor impulse contains a direct consciousness of a motor current, which is the essential element in an act of will; for the kinæsthetic element of the impulse is, with us, extremely slight, if, indeed, it exists at all in ordinary unstudied movements. This view, that the motor impulses, descending from the higher centers to the lower, are accompanied by consciousness, is one that all our experiments have tended to impress powerfully upon us. Yet the tangible, 'statable' evidence for it is extremely slight. It seems to be an unconsciously produced conviction proceeding from a multitude of elusive trifles.

Real automatism, that is, dropping out of consciousness of the other two elements, heard sound, and return sensations from the arm, comes only at intervals and for short periods at a time. But it comes *whenever the attention is sufficiently distracted.* In no case does withdrawal of the attention interfere in the least with the reaction. The writing goes on just the same, but below consciousness. The only exception to this comes on the emotional side. If the story gets very exciting the muscular tension, which is one of the expressions of intense suspense, stops the arm movements entirely, and, of course, with that the possibility of writing words. Also, in very exciting parts, the tendency to write words from one's reading is also increased, but this does not interfere much.

A very distinct stage in the process of becoming unconscious is where we find the word started before we are conscious of having heard it, or we learn the word first from our writing, and then perhaps recall its sound by the memory afterimage; or we are uncertain what word was dictated, and while we are wondering the word is written. Every once in a while the story grows interesting, and we return to ourselves with a start to find that we have been going on writing just the

same. In this connection it is important to notice that the return to consciousness is always from the motor side. We suddenly become aware that our hand is writing something. It is never the sound that recalls us. This, of course, may be an individual peculiarity to a certain extent, and possibly would not be true of everyone. Yet, Miss Stein has a strong auditory consciousness, and sounds usually determine the direction of her attention.

For a long time during these experiments nothing was more marked than the complete failure of automatism as soon as the voice fell below a certain degree of loudness. The moment that happened the writing would not continue without the formation of a motor impulse, usually accompanied by a feeling of effort. This minimal loudness was so near the point of difficult hearing that we could not say whether the feeling of effort really belonged to the identification of the sound, or the formation of the motor impulse.

After long practice this phenomenon disappeared quite suddenly. The minimum loudness took a big drop to a point rather below easy hearing. It now became very much easier not to attend to the dictation, and the intervals of complete unconsciousness lasted much longer, and occurred much more frequently. Our results were now entirely satisfactory and we stopped the experiment.

As to the extent of the unconscious intervals, they frequently extended for five or six words with complete unconsciousness, while the successive occurrence of several such intervals, separated only by momentary flashes of consciousness, was not uncommon.

As to the test for unconsciousness, of course, in the nature of things, the only test can be that of memory. One cannot directly observe unconsciousness. Here it will, of course, be said that there is no proof that it is not merely memory that is at fault. We may be momentarily conscious of these reactions, but forget them. Of course, the same objection can be made to any alleged case of automatism, and the fundamental object of these experiments, to establish an analogy between the acts of the second personality and what is ordinarily called automatism,

is not affected by this objection. There is no proof, save that of memory, for the performance of the so-called 'split-off consciousness' being other than a performance of the primary consciousness, nor for any of the simple reflexes ordinarily called unconscious being really not cases of rapid alternation. Our problem, being purely one of similarity between two well marked systems of phenomena, is independent of the ultimate interpretation of either group. We simply wish to show that what holds for one holds also for the other.

Nevertheless, this question of alternation without memory, versus real unconsciousness, is an important one, and, as we made observations bearing on this subject, it will be well to record them here.

In brief, what we observed was a phenomenon different from true unconsciousness, but corresponding almost exactly to the conception of alternation without memory. The subject was absolutely unable to recall a single word written, but nevertheless felt quite certain that he had been writing, and that he had been conscious of every word as he wrote it. This, in fact, was the general condition of things through the greater part of the experiments, after training was well under way. The same sentence might be dictated to the subject over and over again, and at the end of the series he would not know what it was. Yet not a single instance of what we have called unconsciousness occurred during the interval. Of course, this is not conclusive, for obviously there is memory of some kind even in this case, though not a memory of what was written. But the important point is that real unconsciousness appeared, not as a last stage of this, but as an altogether different phenomenon coming quite suddenly, and under different conditions. The consciousness without memory seems to *approach as its limit*, simply a condition in which the subject has not the faintest inkling of what he has written, but feels quite sure that he has been writing. It shows no tendency to pass beyond this into real unconsciousness. It seems to depend on the lack of associations between the different words—one word going out of consciousness before another has come in to be associated with it. It is facilitated by slow dictation. And conversely real

unconsciousness appears not as a final stage of a gradually decreasing memory, but quite suddenly. It may break into a period of consciousness without memory, and be followed by such again, but it is equally likely to break into a period of complete memory. In either case it comes entirely unheralded by any transition form, and departs as suddenly and silently. It does not seem to depend upon association elements at all—is entirely independent of the speed of dictation up to the limit of writing speed.

This identification of a phenomenon so strikingly in accord with the 'alternation-without-memory' theory, yet so strikingly different from the well known phenomenon of unconsciousness, seems to us to leave little room for reasonable doubt as to the correctness of the common sense view of the unconscious—the view, that is, that it really is unconscious.

This phenomenon of failure of memory, in spite of the presence of consciousness, will at once be recognized as corresponding quite closely to some well known hysterical phenomena. We shall come across more instructive instances of it later on in automatic reading.

It will perhaps be objected to these experiments that the long training required to bring them out destroys their value, for the hysterique does all these things without special training. It will be said that to prove that the second personality uses nothing but habitual brain paths it is scarcely permissible to establish new paths.

But it must be remembered that our training was purely a training of the attention. Our trouble never came from a *failure of reaction*, but from a *functioning of the attention*. It was our inability to take our minds off of the experiment that interfered. From the start, whenever, by good luck, this did happen, the reaction went on automatically. (The exception noted from intense excitement is, of course, of no importance in this connection.) The hysterique has no trouble here, for he is *unable* to *attend* to the sensation, attention to which bothered us. It is his anæthesias which make automatism possible. What in his case is done for him by his disease we had to do by acquiring a control over our attention.

But if there was no real creation of new paths, it will be objected that yet the lowering of the minimal loudness of dictation, so essential to the success of the experiment, was at least an opening up and 'smoothing' of old paths. This is doubtless true, but it must be remembered that training of this kind the hysterique can get during the early stages of his disease. The formation of a second personality is a late development, and sub-conscious acts of an irregular character occur for a long time before the organized second personality appears. During this stage paths which are not yet well worn may be opened up. It will be remembered that in our experiments we found automatism easier when the arm was fresh. When tired it suddenly failed. Apparently, the energy reaching it along the automatic path is no longer sufficient. Produce this backwards now. Imagine an arm in the condition of 'chronic rest' of an hysterical paralysis. Is it not altogether likely that it often acquires great sensitiveness from this, so that stimuli reaching it along the automatic path, not strong enough to produce a reaction in a normally exercised arm, may yet produce a reaction in the hyperæsthetic arm? In this way old paths may gradually be widened, until the second personality emerges —possibly with a sub-conscious hyperæsthesia to trouble some psychical researcher.

Automatic Reading.—This is a very pretty experiment because it is quite easy and the results are very satisfactory. The subject reads in a low voice, and preferably something comparatively uninteresting, while the operator reads to him an interesting story. If he does not go insane during the first few trials he will quickly learn to concentrate his attention fully on what is being read to him, yet go on reading just the same. The reading becomes completely unconscious for periods of as much as a page. In this experiment when well under way, it is the moments of conciousness that are rare. One remembers having read something at the beginning of the paragraph and suddenly finds himself at its end. All between is a blank. One feels that he surely must simply have suddenly let his eyes drop from one end to the other. Often, though the reading is entirely unconscious he is conscious of a confused murmer heard all the

time—the sound of his voice—but it bears about the same relation to his consciousness as the murmer of the stream, beside which one reads on a summer day—a general background of sound, not belonging to anything in particular.

The reading is not entirely lacking in expression, and the pauses are made quite properly. But the tone is usually more monotonous than the reader's normal. Absurd mistakes are occasionly made in the reading of words—substitutions similar in sound but utterly different in sense. The usual suggestibility of the unconscious is shown in a tendency to insert words from the reading which is attended to. (Here it will be noticed appears an automatic path from ear to mouth.) The words read must be familiar for the automatism to work well. Dialect stories do not go well at all.

The eye movements in this experiment are most interesting. The tendency to raise ones eyes from the book one is reading, and turn them on the person one is listening to, is very strong. A compromise is frequently the result. One's eyes are focused at a point a little above the book, and the reading goes on out of the corner of one's eye. Tendencies of this kind, however, are not so hard to overcome as one supposes the first time he tries. Eye movements here seem to be simply a result of attention, not in any sense the thing itself.

The feeling of extra personality appeared here too. Whenever it happened, that is, that the subject after a period of automatic reading suddenly began to *hear* what he was reading, his voice seemed as though that of another person. This effect did not disappear immediately when he began to see the printed words. Not until he had, as it were, 'taken in hand' the process by which printed words pass into speech, did extra-personality disappear from his reading.

When both persons read with equal loudness, each trying to pay attention to the other, the conditions are very different. In the simpler experiments the problem is simply to pay attention to sounds, and not to sight and speech. When both read equally loud, however, this is not enough. It is easy enough to get the reading automatic, but to listen to another person's voice and not to one's own is another matter. Here comes in the distinc-

tion pointed out in the automatic writing between the mere entering of consciousness and the establishment of associations giving memory and meaning. It is not possible to hear only the other person's voice. If the centers for the consciousness of sound are in a condition to respond to afferent currents at all they respond to all, or, at least, do not discriminate except at haphazard. But it is possible to *grasp the meaning* of one only, the other being in the condition of the words written, but not remembered, in the automatic writing. This affords a most interesting field of observation, but as it concerns a different problem from the one in hand I speak no further of it here. It will form part of another series of experiments having as their problem the general relation of attention and memory. These two elements of attention are very distinct. The one a mere attending to certain classes of sensations—a physiological distribution—the other inseparably bound up with the laws of association and the act of thinking things together, the holding before the mind of a general conception which is gradually modified by new information.

4. *Unconscious memory and invention.*—The first experiments in this line were on automatic speaking, and were carried out in connection with the automatic writing at dictation. For this purpose the person writing read aloud while the person dictating listened to the reading. In this way it not infrequently happened that, at interesting parts of the story, we would have the curious phenomenon of one person unconsciously dictating sentences which the other unconsciously wrote down; both persons meanwhile being absorbed in some thrilling story.

In this experiment, as in the automatic reading already described, whenever it happened that the speaker became aware of his dictation solely by hearing his own voice, his voice seemed strange and extra personal. The dictation was of the character that we already had used during the experiments, short, simple words strung along grammatically, but not representing usually any special thought.

Spontaneous automatic writing.—This became quite easy after a little practice. We had now gained so much control

over our habits of attention that distraction by reading was almost unnecessary. Miss Stein found it sufficient distraction often to simply read what her arm wrote, but following three or four words behind her pencil. All the phenomena observed in the writing at dictation were confirmed here—the order of disappearance from consciousness, extra personality, difference between memory and consciousness, etc. Two very interesting phenomena were here observed for the first time.

A marked tendency to repetition.—A phrase would seem to get into the head and keep repeating itself at every opportunity, and hang over from day to day even. The stuff written was grammatical, and the words and phrases fitted together all right, but there was not much connected thought. The unconsciousness was broken into every six or seven words by flashes of consciousness, so that one cannot be sure but what the slight element of connected thought which occasionally appeared was due to these flashes of consciousness. But the ability to write stuff that sounds all right, without consciousness, was fairly well demonstrated by the experiments. Here are a few specimens:

"Hence there is no possible way of avoiding what I have spoken of, and if this is not believed by the people of whom you have spoken, then it is not possible to prevent the people of whom you have spoken so glibly . . . ."

Here is a bit more poetical than intelligible:

"When he could not be the longest and thus to be, and thus to be, the strongest."

And here one that is neither:

"This long time when he did this best time, and he could thus have been bound, and in this long time, when he could be this to first use of this long time . . . ."

In this automatic writing from invention appeared more strongly than anywhere else the fact that the motor impulse is necessary for the feeling of personality. For it was easy here for long periods to get the process in a condition where there was often an expectation of what word would be written, but no *intention* to write it. One watched his arm with an idle curiosity, wondering whether or no the expected word would be written. In these experiments more than in any others did we

feel the need of supposing that conciousness accompanies motor currents. If we wrote without watching what we wrote the writing was rapid and very illegible. By watching the writing, however, or, more correctly, by keeping our eyes on it, for there was no attention to it, the writing was kept even, legible, and at moderate speed. The control of movements by return sensation of sight is thus demonstrated to be an automatic process.

Subconscious exercise of memory.—The subject while his attention was distracted by listening to reading wrote some bit of poetry well known to him. The object was to see whether the memory, though in purely sound and speech terms, would yet go over into writing reactions automatically. The things written were bits of poetry that the subject had often repeated to himself, but never written. The experiment was successful. Its significance is that it shows that an act, to go on automatically, need not have been done before, provided all its elements have been done before. Thus in this case we have a combination of the automatic going over of ideas, or words, into writing reactions, the tendency of words written by the hand to call up in the mind their corresponding sound, and this to call up the next word of the poem which had been memorized in sound terms. The experiment is thus a justification of our general method of splitting up the second personality into its elements, and reproducing them automatically, instead of striving to reproduce the entire phenomenon at once.

Some general characteristics of the experiments.—In all automatism the tendency toward increased speed is marked. Writing tends towards a pace that very quickly tires, reading towards a rapidity that prevents distinct articulation, dictating toward a speed that soon becomes hopelessly fast for the writer. The increase of speed is gradual, and occasional corrections during flashes of consciousness suffice usually to keep down the tendency. The monotony of the automatic reading has its parallel in automatic writing. In the writing at dictation for example it was usually possible for the operator to tell from the way a word was written whether or not it had been entirely non-voluntary. The dropping out of consciousness produced no change in the writing if it was already in the non-voluntary

stage. But the presence or absence of the motor impulse made an enormous difference. The purely non-voluntary writing has a perfect ease and smoothness about it, and a perfect characterlessness. The change is not in the appearance of the writing, but in the hand movements. The pencil movements are more regular in speed, and unaccented, while in the voluntary movements the writing is more jerky.

For distracting attention, literature that is easily followed and emotional in character is by far the best. The advantage of the emotional element is, of course, simply its well-known hold upon the attention. But the need that it shall be something which does not demand a reaction from the intellect of the person is a subtler affair. The mechanism appears to be this, that when the idea cannot be grasped without a conscious effort to keep past facts in mind to compare with present, the attention is kept in a general condition of alertness, unfavorable to the complete neglect of any class of sensations. These general attitudes of attention are very hard to describe, but very interesting and very distinct. One of the most suggestive, for example, was this: We noticed on several occasions that if, for any reason, we had missed any portion of the story, and wanted to go back and read it over again, the doing this stopped the automatic writing. This curious effect we traced to a general feeling of 'keeping things in check' for a moment. The idea of stopping the reading and going back brought the feeling that things must be held in check until this back reading had been done; and this feeling of holding in check expressed itself in stopping the automatic writing, as the intense excitement and suspense did, save that there was no marked muscular tension here.

Anything which favored rapid changes of attention was unfavorable to keeping the attention off the experiment. Stories that moved along smoothly and quickly and called for no reaction but an emotional one were the most favorable. Any *stirring up* of the attention was likely to bring it back to the experiment.

General Summary.—How far now have we gone toward proving our general proposition? We may sum up the experi-

ments by saying that a large number of acts ordinarily called intelligent, such as reading, writing, etc., can go on quite automatically in ordinary people. We have shown a general tendency, on the part of normal people, to *act*, without any express desire or conscious volition, in a manner in general accord with the *previous habits* of the person, and showing a full possession of the faculty of *memory*; and that these acts may go on just as well outside the field of consciousness; that for them, not only volition is unnecessary, but that consciousness as well is entirely superfluous and plays a purely cognitive part, when present. By consciousness we here mean, of course, 'empirical consciousness' or 'conscious consciousness,' as we have called it elsewhere. A possible split off consciousness is expressly excluded from consideration for the reasons given in the introduction.

That the second personality shows, in general, no abilities beyond this will, I think, be readily admitted. But it will be claimed that in exceptional cases the performances of the second personality involve something more—a real judgment and discrimination, or the keeping before the mind of an idea which is gradually elaborated.

Of course, it is not possible to enter into a complete discussion of the theory of these phenomena here. But a few words in defense of the main contention of our experiments will not be out of place. We must leave out at once all the alleged phenomena of spiritualism, as being still under dispute and being equally inexplicable on either of the two theories between which it is the purpose of these experiments to decide. Ruling these out there remains a small number of cases apparently not fully explained as automatic, if our experiments be taken as showing the limit of automatism. These cases may be divided into two groups. The first are those where the reactions seem to be rather too intelligent to involve nothing more than habit and memory. These need not offer much difficulty. Without a full knowledge of the *past history* of the patient, it is not possible to tell just where the limits of habit lie. There is opportunity for large individual difference here, and we must allow for it. What one person would have to think about, another

may be so familiar with as to do quite without thought. It will usually be far more reasonable to suppose special habits for unusual cases than to fly in the face of all analogy and suppose a real second personality present. It must be remembered, too, that real unconsciousness is hard to prove.

Our observations on consciousness without memory show that in many cases the 'second personality' may be helped over a knotty point by flashes of primary personality, and exceptional cases would have to be examined from this standpoint before used to overthrow the automaton theory.

The other group embraces the cases that appear in connection with hystero-epilepsy and post-hypnotic suggestion. The peculiarity of these cases is that instead of one act forming the stimulus for the succeeding one—which would involve nothing but simple association—we have a dominant idea present which guides proceedings. This, of course, suggests the action of voluntary attention. It is like the man who is at work on a problem and voluntarily keeps the problem before his mind until the right associations have been called up by it. The difficulty presented by these cases disappears, however, as soon as we remember that here we have to do with an essentially new element—a *fixed idea*—either the subconscious fixed idea of hystero-epilepsy, or the apparently similar subconscious idea of post-hypnotic suggestion. The presence of these fully explains this apparently voluntary and actively attentive character of the acts without calling in any aid from the voluntary attention. The mechanism of these fixed ideas need not concern us. If it be held that they are kept before the mind by a split of will, this is a theory of fixed ideas, which would have to be considered on its own merits. Our problem is not involved in it essentially.

If, then, it be admitted that these experiments satisfactorily answer the question raised at the outset, if they really show a complete analogy between the performances of the second personality and the automatic acts of normal persons, what general view of hysteria do they suggest?

The answer is fairly obvious. It will be remembered that these phenomena occurred in us whenever the *attention* was re-

moved from certain classes of sensations. Our problem was to get sufficient control of the attention to effect this removal of attention. In hysteria this removal of attention is effected by the anæsthesias of the subject. We *would* not, the histerique *can* not, attend to these sensations. Whatever else hysteria may be then, this, at least, seems most probable. It is a *disease* of the *attention*. An hysterical anæsthesia or paralysis is simply an inability to attend to sensations from this part. The second personality is simply the natural correlate of the anæsthesias, when these have become fixed. When they are variable, irregular subconscious acts form their correlate.

In closing it may be well to sum up a few of the more important generalizations from the work.

There are two kinds of attention, or two manifestations of it. One is *physiological* in its distribution, and determining what classes of sensations shall be brought into consciousness. Its failure means the dropping out of consciousness, for the time, of the particular group of sensations with regard to which it has failed. The other is distributed according to logical and associational elements. Its function is to establish associations among the different elements of consciousness, and to bring out the full meaning of sensations, etc. Its failure means loss of memory and failure of judgment, will, etc., but not loss of consciousness.

In all habitual acts, and acts involving nothing but simple memory, the function of the higher powers of the mind is inhibitive and controlling only, and not productive, for whenever, by failure of attention, the acts are removed from the influence of these controlling and inhibitory powers they go on just the same. Consciousness itself here appears to play a purely cognitive part.

The feeling of personality—that a given act is done by us—always disappears whenever our knowledge of the act is acquired purely by return sensations. Mere fore-knowledge alone is not enough to make the act seem personal; it must be the fore-knowledge or expectation represented by the group of feelings we have called, for convenience, the motor impulse. This motor impulse seems to introspection to be much more than a

mere expectation in sensory terms. It seems to have a feeling background in it, entirely indescribable, in other terms, and perhaps representing a direct consciousness of a motor current from the higher centers to the lower.

The feeling of effort is not essential to self-consciousness. Its function seems to be to bring a center into a more responsive condition. It accompanies movements of voluntary attention apparently.

Hysteria is, at least, a disease of the attention. Its anæsthesias, etc., and their correlated subconscious acts represent the failure of the first kind of attention. The weakened memory and intellect, when it occurs, represents the failure of the second type.

ON THE CONDITIONS OF FATIGUE IN READING.[1]

BY HAROLD GRIFFING AND SHEPHERD IVORY FRANZ.

The increasing part played by reading in the life of civilized man is a striking characteristic of modern culture. In fact, the man of to-day might be defined as a reading animal. The result of this strain upon the eye has been the wide prevalence of myopia, astigmatism and kindred disorders. But the functions which the optic mechanism is called upon to perform are not abnormal; the work of the eye differs only in degree from that for which it is fitted. If the eye were never fatigued, myopia would be rare.

Yet great as is their importance, we have little exact knowledge of the conditions of minimum visual fatigue. Cohn[2], Javal[3] and Weber[4] have treated the subject with great fulness, but their work was largely theoretical. Cattell[5] and Sanford[6] have, however, investigated the subject experimentally, with special reference to the relative legibility of letters.

The conditions of visual fatigue are obviously highly complex. They may be divided into two classes. On the one hand, we have all those conditions which pertain to the individual reader; for example, the time of reading, the position of head and eyes, and personal peculiarities, anatomical and physiological. Opposed to these are certain purely physical conditions. Such are the size and quality of the type, the intensity and quality of the illumination, the color and quality of the paper,

[1] From the Psychological Laboratory of Columbia University. Read in condensed form before the International Congress of Psychology, Munich, August, 1896.

[2] Cohn, *The Hygiene of the Eye in Schools*, Eng. tr., London, 1886.

[3] Javal, *Annales d'Oculiste*, 79–82; *Revue Scientifique*, 1881.

[4] Weber, *Ueber die Augenuntersuchungen in den höheren Schulen zu Darmstadt, Referat erstattet d. grossherz. Ministerial, März*, 1881.

[5] Cattell, *Philosophische Studien*, III.

[6] Sanford, *American Journal of Psychology*, I.

the clearness of the printing, the length of the lines, and the spacing between the letters and lines. It is this latter group of conditions with which we are now concerned.

(1) The Size of the Type.

Weber investigated the relation of the size of type to legibility by finding the maximum rate of reading. He arrived at the paradoxical result that although the rate of reading decreased for very small type it also decreased when the height of letters was over 2 mm[1]. By determining the time of exposure required for perception Cattell[1] studied the legibility of small Latin letters of different sizes, .7, 1.1, 1.8, 2.5 and 5.8 mm.[2] The times found were 3, 1.4, 1.1, .7 and .6 for one observer, and 4, 1.7, 1.3, .9 and .7 for the other. The relation is approximately expressed by an hyperbolic curve.

The investigations of Cattell we have extended and supplemented by different methods. By the first method, which we will call the method of rapid reading, we found the rates at which an observer could read printed matter in large and small type. Two passages of the Bible, each containing 622 words, were used. One observer read one passage A, in large type, and another passage B, in small type, and the next observer read the same passages, reversing the order of the type, reading A in small type and B in large type. The order in which the experiments were made was also reversed for alternate observers. The time was taken by the observer with a stop watch, but recorded without his knowing the result by one of the writers. The observers were mostly students, five being familiar with experimental psychology. The type was Roman, *i. e.*, the ordinary type used in English books. The large type, of which we here give examples, was Pica, 1.8 mm. in height, the small, Pearl, .9 mm. in height. In addition to these experiments we made some in which the time of reading was constant, 1 minute, the number of words read being determined.

Below will be found the ratios of the times and of the number of words read.

[1] *Op. cit.*

[2] Not given by the writer, but calculated by us from other data given.

TABLE I.—RELATIVE TIMES FOR LARGE AND SMALL TYPE.

OBSERVER.	K	A	D_1	B	S	F_1	F_2	F_3	G	H_1	H_2	D_2	Av.
$\frac{T_L}{T_S}$	.77	1.04	.82	.61	.90	.88	.72	—	1.08	.96	1.01	.92	.90
$\frac{W_S}{W_L}$	.88	1.00	—	—	.91	.42	.65	.94	.80	—	—	—	

$\frac{T_L}{T_S}$ = ratio of time required to read large type to that required to read small type.

$\frac{W_S}{W_L}$ = ratio of number of words read in one minute in small type to the number read in large type.

In a few additional experiments the observers read at their natural rates. The resulting ratios $\frac{T_L}{T_S}$ for 4 observers were .87, 1.00, .86 and .81, the average .89, being the same practically as that obtained by the other method.

Thus it takes on the average about $\frac{9}{10}$ as much time to read large type, 1.8 mm., as to read small type, .9 mm. The difference in legibility would probably be much greater were it not that when the small type is read more words can be seen simultaneously. In this way we may explain Weber's paradoxical result. As the size of the letters increases beyond a certain limit the rate of reading will necessarily decrease; but this does not involve an increase of fatigue, as Weber assumed.

By a second method we found the relative number of words seen when exposed for $\frac{1}{20}$ sec. by Cattell's gravity chronometer.[1] Phrases of three and four words were pasted on white strips of cardboard and were shown for the time desired by a falling screen. The greater part of the screen was hidden from the view of the observer by a black sheet of paper with an opening where the letters were to appear. The phrases were cut from the books mentioned, the letters being 1.8 and .9 mm. high. None of the words were of more that two syllables. The same phrases were used for large and small type. There were 54 phrases of 3 words and 54 of 4 words, half in large type and half in small. Thus there were 216 + 162 words in all.

[1] For description of the instrument see *op. cit.*

The experiment was conducted as follows: The observer took his seat in a comfortable chair opposite the instrument and placed his chin upon a rest suitably adjusted, so that his eyes were slightly above the level of the letters exposed, and 30 cm. distant from them. The experimenter (one of the writers) stood behind the instrument so as to adjust the cards with the phrases. When the card was placed the observer fixated a gray cross on the black background of the movable screen directly in front of the letters, and let the screen fall by breaking the current with a Morse key. He then wrote down what he thought he had seen. A dozen or more practice trials were made before beginning the experiments proper. The observer was, of course, ignorant of the phrases that were to be given. Care was taken not to have a phrase already given in one type repeated immediately in another. Of eleven observers six completed only half of the series. We give below the results for the different observers.

TABLE II.—PERCENTAGES OF WORDS SEEN; LARGE AND SMALL TYPE.

	THREE-WORD PHRASES.			FOUR-WORD PHRASES.		
OBSERVER.	S.	L.	$\frac{S}{L}=\lambda$.	S.	L.	$\frac{S}{L}=\lambda$.
H.	.22	.56	.39	.13	.44	.29
C.	.46	.75	.61	.59	.75	.79
T. G.	.29	.75	.39	.23	.60	.38
I. F.	.60	.95	.63	.80	.88	.88
H. G.	.46	.81	.56	.66	.96	.69
P.	.46	.91	.50	.45	.85	.53
L.	.10	.54	.18	.18	.32	.56
R. G.	.76	.79	.96	.48	.68	.70
S.	.12	.47	.25	.12	.39	.31
A.	.68	.78	.87	.55	.69	.79
S. F.	.43	.85	.51	.59	.81	.73
Average			.53			.60

Vertical columns S and L give percentages of words seen for small and large type (.9 and 1.8 mm. high).

Vertical column $\frac{S}{L}$ give ratios in per cent. or the relative legibility λ of small and large type.

With the observers whose initials are given in block type the full set of experiments (108) were made, only 39 being made on the others.

In taking the average the values of λ for these five might be weighted. This would change the averages somewhat.

From the above table we see that on the average but little more than one half as many words were seen in small type as in large type. Individual variations are great, but these variations are probably not due to an appreciable extent to individual differences in the relative legibility of large and small type. For good observers the same difference in legibility would give different values of λ.

This theoretical conclusion is verified by the experiments. By arranging the observers in two groups according to the percentages seen, the values of λ is for the better observers in all cases lower than that of any of the four poorest observers.

A few experiments were made with 21 two-word phrases printed in very large type (4+ mm). The percentages of words seen correctly by three observers, together with the averages of the same observers for 1.8 mm. type as found from the table above given are as follows:

	Large	Very large
P.	.88	.93
L.	.43	.64
S.	.43	.70

Thus the legibility as shown by this method appears to increase regularly with the size. But since the number of words brought within the field of distinct vision decrease with the size, the relation is quite complex.

A few phrases (15) of two words each were used with the others. The percentages for two, three and four-word combinations were found to vary but little with the number of words.

From the table it will be seen that the values of λ were about the same for phrases of three words as for those of four words, the averages for phrases of 2, 3 and 4 words in small type being .42, .41 and .43.

In the above experiments the paper was not exactly the same for large and small type, being slightly grayish for the small type and of a more yellowish tint for the large. To eliminate this source of error, phrases of four words in large and small type were printed on the same white paper. From 200 experiments (800 words), 100 on S. F. and 100 on H., we found the following percentages of words seen:

	S	L	$\frac{S}{L}=\lambda$
H.	.12	.32	.37
S. F.	.83	.90	.92

The values of λ correspond quite closely with those previously found for the same observers, .88 for F. and .29 for H.

A modification of the preceding method was used by determining the time words composed of letters of different sizes had to be exposed in order to be seen. This we will call the time of exposure method. The same apparatus was used as before, the time of exposure varying with the extent of opening of the screen. This time can be determined to about $.15\sigma$, σ being .001 sec. The words were of not less than 5 letters, nor over 2 syllables, on white paper. The type, as here shown, was six point and 'eleven point,' .8 and 1.6 mm. high. On account of the preliminary practice necessary there were but three observers, two being the writers. The experiments were conducted in the same general way as those just described. The experimenter tried first very small times, increasing the time until the stimulus was perceived approximately 50% of the time. Then other words were shown which the observer had not seen. As the percentage seen tends to increase very rapidly from 0 to 100 (theoretically 99+), it was generally easy to determine at one sitting the time required either directly or by estimation from the percentage seen. The times of exposure found thus are now given in thousandths of a second.

TABLE III.—TIMES OF EXPOSURE FOR DIFFERENT SIZES OF TYPE TO BE SEEN.

OBSERVER.	L.	S.	$\frac{L}{S}=\lambda$
G.	1.6	1.9	.84
F	1.1	1.5	.73
"	1.3	1.7	.76
H	2.0	2.8	.71
"	1.6	2.5	.64
AV.			.73

L and S denote the times of exposure necessary for large and small type respectively, .8 and 1.6 mm.

$\frac{L}{S}$ or λ is the relative legibilty measured by this method.

The two values of L and S for F and H are for different days. The time of exposure seems to vary in the same individual.

From the above results it appears that the large type, 1.6 mm., requires about ¾ as great a time of exposure as the small type of half the height, .8 mm.

In the last two sets of experiments a few observations were made, which though not bearing on the special problems under investigation are yet of psychological interest. Observers generally failed to see any of the letters making up a word when they failed to perceive the whole word. There were, however, individual differences, some persons often seeing one or two letters only. At times an observer saw combinations without sense, though he knew such combinations were not given. In the time-of-exposure experiments the observer was at times conscious of perceiving letters without knowing what they were. Occasionally the observer had an impression that a given word was present, when the letters had not appeared distinctly. More often some letters were distinct, and he guessed the word, or else the whole word was distinct. One of the writers had a marked tendency to see again what had been given before, even when he knew that the word was not repeated. One of the observers, H., seemed to be an exception to the rule that one sees all letters exposed or none at all except within very small range of time. Some days it was very difficult to find the time required for this reason. But perhaps the most important phenomenon observed was the illusory perception of a word, the letters appearing distinct when not present. This has been already noted by Cattell and also by Münsterberg. The theoretical importance of this lies in the support which it gives to the hallucination theory of perception. The representative processes in perception seem to attain to the sensory vividness of true hallucinations. This does not, however, appear to take place in every instance, for F. seemed at times to see some of the letters and to infer by ordinary processes of association that a certain word was present.

To obtain more extended results and confirm those obtained by Cattell, by the time-of-exposure method, we determined the intensity of illumination necessary for the reading of letters of different sizes. The letters were printed in the simplest kind of type, commonly called Block. Two cards were, how-

ever, covered with words in Roman type, .8 and 1.6 mm. in height.

The observer sat in front of a stand from a projecting piece of which was suspended a small pendulum making a vibration in ½ sec. The pendulum swung in front of a screen having an opening where the letters to be seen appeared. The letters were, of course, shown ½ sec. The letters were posted on cardboard strips and these were placed in slits. The paper was the same for the different sizes, pure white. The slits were arranged so that the length of the cardboard exposed was either 15 or 3 mm., according to the size of the letters. For the two largest sizes, and also for the cards on which the words in Roman type were shown, the large area was used. The object was to show only one or two letters at a time, except when the Roman type was used, when a larger number was seen. A black screen in front of the pendulum with the necessary opening served to prevent distraction of the observer by the movement of the pendulum.

The observer's eyes were kept at a constant distance (30 cm.) from the stimulus by means of a chin rest. The light was that of a hooded petroleum lamp found to be fairly constant, shining through a square of ground glass 5 x 5 mm. The light emitted was approximately .02 candle power. The lamp was in a movable box sliding on wheels in iron grooves. Precautions were taken to avoid errors from reflected or diffused light. The letters used were in combinations of one to four words in one horizontal line. They were taken from a printer's sample book. The median plane of the observer was approximately perpendicular to the plane of the cardboard to be seen, and the lamp could be moved only in a straight line, making an angle of 45° with the plane of the cardboard.

With this apparatus after the observer had remained in the dark room long enough to avoid errors from adaptation (20 to 30 min.), the experiment was made as follows: A card with letters to be exposed was placed in the slit by the experimenter (one of the writers). The observer pushed back the pendulum to a fixed support with his hand, fixated a pencil cross on the cardboard piece fastened to the pendulum directly in front of

the letters to be seen, and then let the pendulum swing forward, observing the letters as they were shown. As the pendulum swung back it was caught by the observer with the left hand and fixed with a catch. He then moved the lamp nearer with the right hand. At first this was done by the experimenter, but with less convenience and economy of time. This was repeated until the observer was quite certain he could perceive the letters correctly when exposed but once. The distance of the light from the letters was then read off on a scale. The square of the reciprocal of this distance represents the relative intensity of the illumination. The readings were, of course, taken by the experimenter. For this purpose we used the light from a small candle inside a blackened box shining through a cylindrical tube. Two or three determinations were generally made at one sitting for each of the variables under investigation, including several in addition to the type. Variations in the results made it necessary to average the records of some days separately, as given in the second horizontal columns for F and H.

We give below the average values of T, the illumination threshold[1] for reading in terms of one candle-meter (C.M.), or the light of a standard candle at a perpendicular distance of one meter.

TABLE IV.—ILLUMINATION THRESHOLDS FOR DIFFERENT SIZES OF TYPE.

OBSERVER.	N	H = .9		H = 1.6		H = 3.1		H = 6.0		h = .8		h = 1.6	
		Av	MV	Av	MV	Av	MV	Av	MV	Av	MV	Av	MV
G	10	27	.02	.12	.01	.042	.003	.014	.001	.36	.04	.14	.01
F	6	.24	.02	.08	.01	.028	.007	.010	.001	.22	.02	.12	.02
"	3	.17	.03	.045	.004	.018	.002	.008	.001	.13	.01	.05	.00
H	5	.077	.014	.035	.007	.014	.001	.003	.000	.19	.01	.07	.00
"	3	.19	.02	.09	.003	.043	.003	.009	.001	.35	.03	.13	.02

H = height of Gothic letters in mm.

h = height of Roman letters in mm.

N = number of determinations upon which average is based.

Av = average.

MV = mean variation.

[1] Calculated by the formula $T=\frac{\lambda \cos\theta}{d^2}$ where λ is the candle power of the light, d the distance of the light from the object, and θ the angle made by the normal to the surface.

A graphical representation of the results is shown in the accompanying figure. The ordinates give the intensity of illumination in candle-meters, and the abscissas the height of the letters in tenths of millimeters.

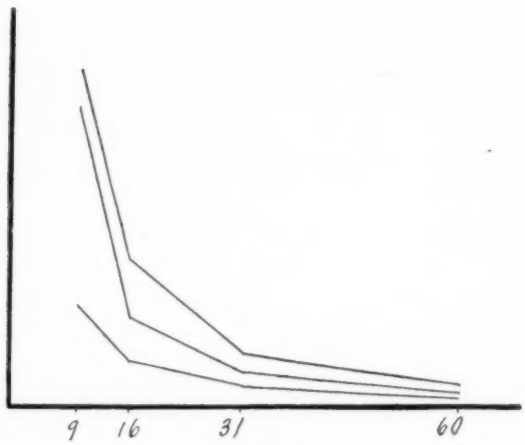

FIG. 1.

The curves resemble rectangular hyperbolas, the values of the variables corresponding roughly to the equation,

$$(s-k)\, i = k_1,$$

k and k_1, being constants depending upon the individual. Assuming such an equation we may infer that after the size of the type has reached a certain limit the increase of size is in direct proportion to the decrease of illumination. The fatigue coefficient increases slowly until the size of the type decreases to about 2–3 mm., after which its increase is more and more rapid. The lowest limit to the size of type in common use should be 1.5 mm. The same conclusion may be drawn from the experiments of Cattell already mentioned.

(2) THE QUALITY OF THE TYPE.

On theoretical grounds it may be assumed that the legibility of letters decreases with increasing complexity of structure. From this point of view German type is open to serious criti-

cism, and even our Roman type might evidently be much improved. Some of our letters have unnecessary features and they are as a rule much more complex in structure than those printed in the so-called Block type. Many letters, such as c and e, are easily confused, and there are decided differences of legibility. These differences are, indeed, slight and difficult to determine. By finding the percentage of times each letter was seen when exposed for 1σ, more or less, Cattell[1] found the order of legibility of the small letters to be: d k m q h b p w u l j t v z r o f n a x y e i g c s. There seemed to be, however, individual differences. Sanford[1] by a different method found a somewhat different order of legibility.

In the writers' experiments, which were made only by the most delicate method, that of the illumination threshold, the following styles of type were used: Roman (small letters), that used universally in England, America and southern Europe for books and newspapers; German, or that used in Germany; Block, in which the letters are of uniform thickness and of the simplest shape, much like Roman capitals. Two styles of Block were used, as here shown; in one the letters being quite THICK, .5 mm., whereas in the OTHER they were .15 mm. Besides the ordinary Roman letters there were two other sets in semi-Roman type; one, **Roman II.**, having very thick and very thin lines, .05 to .5 mm.; the other, Roman III., being somewhat like the plainer Block and of uniform thickness, about .2 mm. The size of the letters was practically constant for the different groups, 1.5 mm. in height, there being, however, slight variations, to .1 mm. in the individual letters.

We give now the results in tabular form. The figures mean the same as in Table IV. The results for F. and H. are given in 2 columns on account of a variation in sensibility which made it necessary to average the results of the earlier experiments separately.

[1] *Op. cit.*

TABLE V.—ILLUMINATION THRESHOLD FOR DIFFERENT KINDS OF TYPE.

		ROMAN I.		ROMAN II.		ROMAN III.		GERMAN		BLOCK THIN.		BLOCK THICK.	
OBSERVER.	N	AV	MV	AV	MV	AV	MV	AV	MV	AV	MV	AV	MV
G.	10	.22	.02	.12	.02	.18	.02	.21	.03	.20	.02	.09	.01
F.	6	.14	.01	.12	.02	.13	.01	.15	.02	.13	.01	.07	.01
"	3	.11	.01	.06	.001	.08	.00	.12	.01	.10	.00	.06	.00
H.	5	.10	.01	.06	.01	.12	.02	.12	.02	.10	.02	.04	.00
"	3	.22	.03	.15	.03	.22	.02	.24	.02	.22	.01	.10	.00

From the above table we may calculate the relative legibility λ of the different styles of type; λ of course being the reciprocal of the illumination threshhold given above.

The values of λ are now given.

TABLE VI.—RELATIVE LEGIBILITY OF TYPE, THAT OF ROMAN BEING I.

OBSERVER.	ROMAN II.	ROMAN III.	GERMAN.	BLOCK THIN.	BLOCK THICK.
G.	1.8	1.2	1.0	1.1	2.4
F.	1.1	1.1	.9	1.1	2.0
"	1.8	1.4	.9	1.1	1.8
H.	1.7	.8	.8	1.0	2.5
"	1.5	1.0	.9	1.0	2.2
AV	1.6	1.1	.9	1.1	2.2

From the above we see that, contrary to our expectation, the difference in legibility between Roman and German type is relatively slight. Thin hair lines, if accompanied by thick lines, do not seem to diminish the legibility, Roman II. requiring nearly half as much light as Roman I. The complexity of the letters, within the limits here studied, does not seem to have decided effect on the legibility, for the value of λ for thin Block is about the same as for Roman. The greater legibility of Block type is due almost entirely to the thickness of the letters, as shown by these experiments. On the other hand, if a part of the letter is thick it is quite legible, even though thin hair lines are frequent. It is, however, probable that type such as Roman II. is

more fatiguing than the results indicate. It may be possible for the mind to perceive certain objects with fatigue when other objects are either perceived without appreciable fatigue, or not perceived at all.

(3) The Distance Between the Letters and Lines.

The horizontal distance between the letters has been said by Javal to be of great importance. Certainly a word printed so that these distances is increased .8 to 1.3 mm. appears to be much more distinct. The effect of an increase in this spacing is here shown. But twelve experiments on three observers by the illumination threshold method gave negative results. We must conclude then that the spacing commonly used is quite sufficient. Greater spacing would, of course, be more expensive, and the decrease of fatigue not as great as might be brought about in other ways.

As regards the vertical space between the lines, technically called 'leading,' a slight effect on legibility was found when the distance with Pearl type, .8 mm. high, was increased from .8 to 1.3 mm. The illumination threshold method was used, and the experiments carried on simultaneously with the preceding. The following results were obtained:

Table VII.—Illumination Threshold for Type Leaded and not Leaded.

	.8 mm.		1.3 mm.		?
	Av	MV	Av	MV	
G.	.40	.03	.36	.04	.90
F.	.25	.01	.22	.02	.88
"	.12	.01	.12	.00	1.00
H	.12	.01	.09	.01	.75
"	.35	.02	.27	.01	.77
Av					.86

Thus the average relative legibility of unleaded type to leaded type, as measured in this way, is about .9.

(4) The Intensity of Illumination.

Although the variation in the intensity of diffused daylight in a well lighted room is known to be very great, even when the other conditions such as time and place are constant, being roughly from 50 to 1500 candle-meters,[1] no results were obtained for variations in legibility due to this variation by the two gravity chronometer methods and the method of rapid reading. The problem is, however, difficult to investigate in this way by reason of the marked daily variations in individual sensibility. It was necessary, therefore, to use artificial light of low intensity. The relation of the intensity and the legibility under these conditions has already been studied by Cattell by the time of exposure method. Using the light of a petroleum lamp (about 10 candle power) 18 cm. distant at an angle of 55° as the unit of illumination, *i. e.*, about 260 c. m., the times of exposure for the intensities 1, $\frac{1}{4}$, $\frac{1}{16}$, $\frac{1}{64}$, $\frac{1}{256}$, were 1.4σ, 1.7, 2.5, 6. and 20.

In order to supplement the work of Cattell we determined the maximum rate of reading for different intensities of illumination. It had already been found by Weber[2] that for low intensities the rate of reading varies with the illumination. Our experiments were made in the following manner: The book to be read, the Pearl type Bible already mentioned, was fastened on a wooden stand so as to be in front of the observer, and making an angle of 45° with the rays of light. The light used was a standard candle placed inside a blackened box. The conditions were such that the light came from behind the observer and to his left. The observer read one column as fast as possible, recording the time with the stop watch. With the lowest intensity, however, on account of fatigue, but half a column was read, the time being doubled for the whole column, The observer, it should be added, remained in a dark room long enough to avoid errors from adaptation. The experiments were made on one day by each observer. Below are given the results in seconds for the different distances in meters and the relative intensities in candle-meters.

[1] Cohn, *op. cit.*
[2] Weber, *op. cit.*

TABLE VIII.—TIME OF READING AT DIFFERENT INTENSITIES.

OBSERVER.	DAY-LIGHT.	¼ M. 11.2 C. M.	½ M. 2.8 C. M.	1 M. .7 C. M.	1½ M. .35 C. M.	2 M. .17 C. M.
H G	35	36	36	46	63	110
K	45	44	39	53	83	120
F	47	51	52	69	100	170
G	47	49	59	72	130	—
S	29	29	35	48	—	—

In these experiments the rate of reading does not appear to be appreciably affected by a decrease of illumination within a very wide range, the intensity of good daylight being about 500 times as bright as the lowest intensity here used, with which the rate of reading was not appreciably increased. We conclude then that within wide limits such as those of ordinary daylight variation in the intensity of illumination is not attended by great fatigue. But when the illumination decreases to a certain point, not far from 3 C. M., the fatigue becomes excessive. This is shown by the fact that very slight differences in the rate of reading are caused by conditions of great fatigue, an increase of about $\frac{1}{9}$ in the time of reading corresponding to decrease in the illumination threshold of 70 per cent.

The above experiments correspond quite well with those of Cattell by the time-of-exposure method. His results show that the fatigue coefficient increases very rapidly as the illumination decreases below approximately 4 C. M. His experiments also show that the fatigue coefficient is appreciably greater for the lamp light, about 250 C. M., than for daylight, and that it increases as the illumination is further decreased.

(5) THE QUALITY OF THE ILLUMINATION.

The use of artificial light has long been recognized as an important cause of visual fatigue. This fatigue may be partly ascribed to the conditions of intensity, the light of a good petroleum lamp at convenient reading distance being less than that of good daylight. We, therefore, tested the effect of artificial light of high intensity by using the light from an incandescent Welsbach gas burner giving clear, white light, 35 candle power at 25 C. M. and 45°, about 400 C. M. The times of

exposure required for perception by the writers were found to be as given below.

	Small Type.		Large Type.	
	Welsbach.	Daylight.	Welsbach.	Daylight.
G	1.8	1.9	—	—
F	1.1	1.5 1.7	.8	1.1 1.3

These values are thus smaller, rather than larger, than those already found for daylight. We must suppose the decrease in time to be due to daily variations. The above measurements were made on one day, and the perceptive and retinal processes of F were more than usually delicate. The smallest time found $.8\sigma$, is about as small as any found by Cattell in all his experiments. It is evident, therefore, that with sufficient intensity of white artificial light the legibility of printed matter may be as great as in good daylight.

Gas light and lamplight have, in addition to their frequent unsteadiness, the disadvantage of a yellow color. Since, as will be seen later, yellow paper is unfavorable for reading, yellow light causing the paper to appear yellow must also be a source of fatigue.

(6) The Quality of the Paper.

If the paper used reflects very little light and is of such a quality that letters can be well printed, the exact hue is probably of little importance, provided a large quantity of light be diffused. But if the absorption be so great that the paper appears grayish, letters printed on it will not be so legible by reason of the lessening of the contrast between the letters and the background.

In experiments made by the different methods already described we used non-reflecting clear white paper and gray paper, technically called news-paper, the same as that used by many newspapers, only slightly darker. By the color-wheel method it was found that the white paper used had to have 30 per cent. black mixed with it to give a gray corresponding to this. Its relative luminosity was therefore about .70. Specimens of red and yellow paper were also used, the red corresponding to the spec-

trum color just to the left of Frauenhofer's line C, and the yellow that to the right of line D ($\frac{1}{6}$ of the distance to line E).

Experiments by the method of the percentage of words seen on one observer with 11-point type gave negative results, the percentages of words seen out of 150 being 32 per cent. and and 31 per cent., the same for white paper as for the newspaper. Of small type words, 6-point, given at the same time, the same observer, H., saw but 12 per cent.

By the time-of-exposure method, however, different results were obtained. Below are the times found for two observers.

	White.	News.	Yellow.	Red.
G.	2.8	4.0	4.0	—
F.	1.2	1.7	2.5	4.0

Thus the time of exposure is considerably longer for gray tinted paper, as well as red and yellow paper, than for white. The explanation of the greater legibility of the letters on white paper over those on the red and yellow is the same as for the gray. Color quality is not independent of intensity, white being essentially brighter than yellow, which in turn is brighter than red.

The illumination method was also applied to the study of the fatigue effect of white paper and gray newspaper. The letters were not read independently in these experiments, but in words. Upon the paper exposed were 10 to 12 words in 3 lines.

The values of the illumination threshold were as follows:

TABLE IX.—ILLUMINATION THRESHOLDS FOR WHITE AND GRAY PAPER.

OBSERVER.	G		F		F_1		H		H_1	
	Av	MV	Av	MV	Av	MV	Av	MV	Av	MV
W = White	.10	.01	.10	.01	.06	.01	.04	.00	.10	.02
N = News	.20	.02	.16	.02	.08	.01	.07	.00	.23	.01
$\frac{W}{N} = \lambda$	.50		.62		.75		.57		.43	

According to these results the gray tinted newspaper required about twice as much illumination as the white. This is

somewhat more than might be expected from the relative absorption powers of the papers, but the quality of the printing varies with the paper, not being quite so clear on the newspaper.

Summarizing briefly our results we conclude that the size of type is the all important condition of visual fatigue. No type less than 1.5 mm. in height, that in which this article is printed (eleven point), should ever be used, the fatigue increasing rapidly even before the size becomes as small as this. The intensity of illumination is apparently of little consequence within the limits of daylight in well lighted rooms. Very low intensities, less than from 3 to 10 candle-meters, are sources of even greater fatigue than small type, and 100 C. M. may be considered a safe limit. Yet the illumination in German school rooms has been found to be frequently less than 2 C. M. White light rather than yellow light should be used for artificial illumination. The form of the type is of less importance than the thickness of the letters. White paper should be used, though it is possible that the greater amount of light reflected from pure white paper may cause some fatigue. Additional 'leading' or spacing between the lines, is also desirable.

THE ACCURACY OF OBSERVATION AND OF RECOLLECTION IN SCHOOL CHILDREN.[1]

BY SHEPHERD IVORY FRANZ AND HENRY E. HOUSTON.

Whether accuracy of observation and of recollection differs at different periods of our lives is a problem suggested by Prof. Cattell's paper on this subject.[2] In order to study this subject, questions similar to those used by Prof. Cattell and by Mr. Bolton, with the changes necessary for time and place and for the age of the scholars, were asked the pupils of the Horace Mann School, New York City, and of the Paterson, N. J., High School.

The following were the questions used: (1) What was the weather a week ago to-day? (2) Two weeks ago? (3) Which way do the seeds in an apple point? (4) How many years ago did George Washington die? (5) How many feet is it from the schoolhouse door to the corner of the street? (6) How many seconds does it take you to walk this distance? (7) How many times have you entered the schoolhouse gate (or door) since vacation? (8) How many ounces does this book (showing a text-book used by the class) weigh? (9) Draw on a scale of one inch to twenty feet, a ground plan of the lower hall.

The accompanying Table[3] gives the percentages of correct answers or the average estimation together with the average residual for the two schools.

[1] From the Psychological Laboratory of Columbia University.

[2] The Accuracy of Recollection, J. McKeen Cattell, *Science*, N. S., II., 761–766, 1895. See also The Accuracy of Recollection and Observation, F. E. Bolton, PSYCHOL. REV., III., 286–295, 1896.

[3] Owing to the fewness of answers in some grades it was thought best to combine the several grades of the H. M. S. as follows: I., II., III., IV., V., VI., VII., VIII., High, thus making about forty or fifty answers in each group.

The figures in the Table marked with a cross (†) denote the actual magnitude as used for the Columbia and Wisconsin Students.

As the books used as standards of weight were of different weights, we

TABLE I.

	ACTUAL M'GN'T'DE.	H.M.S. I.II.III.	H.M.S. IV.V.VI.	H.M.S. VII.VIII	H.M.S. HIGH.	H.M.S. TOTAL.	P.H.S. I II.III.	COLUMBIA.	WISCONSIN.
Age.		7–9.	10–12	13–14	14–17	7–17	14–17	—	—
No. of Answers.		56	63	48	34	201	325	56	92
Weather, 1 wk. previous.	H. M. S. clear. P. H. S. cloudy.	40%	81%	95%	85%	78%	4%	11% stormy clearing.	32%(?) stormy.
Weather. 2 wks. previous.	H. M. S. clear. P. H. S. stormy.	34%	49%	65%	65%	53%	29%	—	—
Direction of Apple Seeds.	H. M. S. P. H. S. =	51%	52%	26%	51%	45%	49%	41%	49%
Yrs. since W's death.	Av.Est. H. M. S.	97	87	97	99	95	102	—	—
	Av. Res. P. H. S. 96.	54	33	12	8	26	13	—	—
Distance in feet.	Av. Est. H. M. S. 400	160	183	167	226	181	197	356	276
	Av. Res. P. H. S. 260	120	150	74	93	118	97	179 [310]	— [450]
Time in seconds.	Av. Est. H. M. S. 80	65	82	97	97	84	70	66	182
	Av. Res. P. H. S. 55	45	52	61	49	54	45	36 [35]	— [160]
Frequency.	Av. Est. H. M. S. 100*	179	252	122	152	183	452	4022	—
	Av. Res. P. H. S. 180	162	185	38	76	131	314	2669 [?]	—
Weight in Ounces.	Av. Est. H. M. S. 10	7.8	7.6	6.5	6.0	7.1	12	17	20.5
	Av. Res. P. H. S. 14	4.5	4.1	2.4	2.4	3.5	5.5	5 [24]	— [24]
Proportion, Width, Length.	Av. Est. H. M. S. 10.2	—	8.7	7.8	8.5	8.3	1.14	—	1.7
	Av. Res. P. H. S. 1.74	—	3.9	2.5	3.6	3.	.30	—	— [2.0]
Length in mm.	Av. Est. H. M. S. 211.	—	116	145	158	141	105	—	6.
	Av. Res. P. H. S. 118	—	39	35	29	37	—	—	— [9.6 in]
Width in mm.	Av. Est. H. M. S. 13.	—	15	21	23	19	87	—	3.5 in.
	Av. Res. P. H. S. 16	—	6	8	8.7	7.8	—	—	— [4.7 in]

Taking the figures more in detail, it will first be noted that the H. M. S. has a much larger percentage of correct answers to the two weather questions than any of the other schools. This is no doubt due to the fact that the weather on the two days about which the pupils were asked was 'clear,' and as we have more clear days than other kinds we should expect an increase according to the probability. Not knowing the probability of this and the other kinds of weather, we cannot compare the other schools, but considering the H. M. S, alone it seems likely that accuracy of recollection increased with age.

In the next question, however, this is not the case, for the younger scholars in the H. M. S. had the same percentage correct as the older, and a trifle greater percentage than the College students. Some chance variation caused a decrease to 5 per cent. in the seventh grade, whence the total for that group (VII., VIII.) was reduced to 26 per cent.

In the quantitative estimations it will be noticed that, like the College students, the younger children underestimate weight and size (proportion) and overestimate time. They also overestimate frequency and with the Wisconsin students underestimate distance and size (length of building). The H. M. S. and the P. H. S. overestimated the breadth of the hall or building, while the Wisconsin students underestimated the corresponding magnitude. In these estimations, however, there seems to be no regular increase or decrease in accuracy, except in the cases of 'weight,' 'length,' 'width,' and 'time.' Taken as a whole, however, the older scholars are more accurate than the younger. This is shown, also by the average residuals, which for the

have here reduced the estimations, taking ten ounces as a standard. The validity of this procedure is somewhat doubtful, but it was necessary in order to make any comparison of the grades. We, however, give here the actual magnitudes, the average estimations, and the residuals for the several grades.

TABLE IA.

	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	High. 18. I.	High. 18. II.
Magnitude.	—	12.5	10.5	10.5	13.5	10.5	14.	19.5		
Av. Est.	—	12.5	6.	8.7	8.7	7.8	10.	11.	10.4	12.
Av. Res.	—	5.	4.5	5.4	5.	3.7	4.	3.4	3.7	3.6

older scholars are considerably smaller than for the younger. The questions are so complex in themselves, all including observation, with errors of judgment, and memory with its errors, that no general conclusion can be drawn.

Accuracy according to Sex. From the following Table showing the percentage of right answers and the average esti-

TABLE II.

	H. M. S.		P. H. S.		WISCONSIN.	
	BOYS.	GIRLS.	BOYS.	GIRLS.	BOYS.	GIRLS.
Weather, 1st wk	74%	81%			19%	54%
Weather, 2d wk	49%	57%			—	—
Apple seed	48	43			50% [only part]	46%
Yrs. since W.'s death . . .	95 (96)	91	89. (96)	102.	—	—
Distance	231 (400)	151	189 (260)	196	296 (450)	261
Time.	72 (80)	90	46. (55)	67.	177 (160)	187
Frequency	191 (100*)	178	505 (180)	468	—	—
Weight	7.8 (10)	6.7	11. (14)	12.	22.8 (24)	19.8
Proportion	9.7 (10.2)	7.0	1.26 (1.74)	1.08	—	—

N. B.—The actual magnitudes are shown in parentheses.

mations for the H. M. S., the P. H. S. and the Wisconsin students. One sees that the girls remember the weather better than the boys, but that the estimations of the boys for distance, time and proportion are nearer the standard. The boys in the H. M. S. came nearer to the date of Washington's death, while the boys and girls of the P. H. S. were about equally correct. With weight the H. M. S. boys again came nearer, while the girls of the P. H. S. were more exact. With frequency the girls in both cases were more correct. The general

conclusion to be drawn is that in quantitative measurements the boys are more exact. This is also what Mr. Bolton found with the Wisconsin students.

Relation of Confidence to Accuracy. When the students were asked the questions they were told to denote by the letters A, B, C or D, respectively, whether they were sure their answers were correct, fairly confident, doubtful, or if their answers were only a guess. The following table gives the average estimation when the students were confident (A and B), and when they were doubtful (C and D).

TABLE III.

	YRS. SINCE W'S DEATH.	WEIGHT IN OZ.	DISTANCE IN FT.	OCCURRENCE.	TIME IN SECONDS.
H. M. S. A. and B.	88.5		152	205	91
H. M. S. C. and D.	138. (96)	(10)	285 (400)	213 (100)	101 (75)
P. H. S. A. and B.	100	12	203	386	
P. H. S. C. and D.	104 (96)	11.5 (14)	214 (160)	475 (180)	(55)

Here, too, the evidence is conflicting and no general conclusion can be drawn. In the estimation for years since W's death, and for number of occurrences the more confident answers are nearer the truth. When we look at the estimation for distance, however, we see that the two schools disagree. The small difference, too, between the estimates in some cases (*e. g.*, years P. H. S., distance P. H. S., occurrence H. M. S.) together with a large variation (in most cases one-third of the average estimation) makes it unwise to hazard any conclusion.

It was found that scholarships did not at all influence the results. Those classed as the best students estimated as wildly as those considered the worst; those considered as of medium ability were a little more accurate than the two extremes.

DISCUSSION AND REPORTS.

REMARKS ON PROFESSOR LLOYD MORGAN'S METHOD IN ANIMAL PSYCHOLOGY.

The method of animal psychology has generally been so indefinite and uncritical, and the results so unsatisfactory, that many psychologists must often have felt that it is quite premature to enter the field at all till we have some clearer basis in a knowledge of what lies nearer us—human psychology. However this may be, there are certainly now a number of able investigators in this province, and not the least of these is Professor Lloyd Morgan. In these remarks I wish to take up certain points made by him in the suggestive book entitled 'An Introdution to Comparative Psychology.'

In the first place it is an obvious remark that the proper method of experimenting on animal intelligence should not be an exciting one, as disturbing to cool deliberate action. It is curious that Professor Morgan acknowledges this (p. 260) and yet brings up the instance there cited, the bone-swinging experiment, as evidence against perception of relations. We may criticise in the same way those experiments upon which he lays great stress as evidence against the perception of relations, namely, the fox terrier pup carrying a stick through railings, that here the activity is of too exciting a nature to be favorable to intelligent adaptation. Yet it may be urged that even herein that the dog is constantly changing his grip, there is evidence that he perceives the necessity of another way than the present. An unchanging stubborn bull dog hold would be less intelligent. The method of trial and error is a real method and is learned to be such, and with a consciousness that his present hold is a bad one he shifts his grip. There is for him a *how* but it is *any how*. We would suggest that young children be tested with the stick-railing experiment. But for the testing perception of space relation we think the spectacle of a cat on a wet day, looking down from a high point before it jumps and makes its way to the house is more suggestive. Here is an opportunity for cool deliberate inspection and comparison, and the cat appears to do this. It seems to pause and judge distance with reference to its ability at jumping, to estimate the shortest path to its destination, and the rela-

tive wetness of different ways. It passes its eye from point to point, and picks its way; and it gives the signs of perception of space relations so plainly that if we saw similar action in a man we should unhesitatingly ascribe such consciousness to him.

With respect to the perception of relations, Professor Morgan adduces on the negative side an observation which illustrates to a certain degree an exciting method, but also the most common fault, an incomplete study of facts. I allude to the story (p. 301) of a dog which after repeatedly chasing a rabbit in vain, the rabbit escaping in a drain, at length made straight for the drain and headed off the rabbit. Upon this brief account two theories of action at once suggest themselves, first (Wilson), the dog may have consciously taken the shortest cut to head off the rabbit, second (Morgan), the dog in following the rabbit, fast disappearing toward the well-known drain, has the association of rabbit and drain at length so predominant that he follows this line of vision—straight line—at once to the drain. In this last case the idea of rabbit entering drain becomes stronger motor impulse than the sight of rabbit running. One objection to this second interpretation is that this mere ready made association of rabbit and drain could only send the dog along the *usual* path to the drain, and this usual path is the rabbit's. However, and we wish to lay special emphasis on this, both the above interpretations are speculations which are, perhaps, worth making, but only as helping to scientific study of the facts. Such a study would mean this: that the master of the dog is a competent dog psychologist, thoroughly acquainted with dogs in general, and this dog in particular, in all his ways and expressions, and yet not biased for the dog—masters like parents are liable to be prejudiced in favor of their charges—and that he sees the dog clearly when the making for the drain was *first* accomplished; then, judging from the method of expression of the dog at that instant whether there was evidence of hesitation or deliberation, attain a competent judgment of the case, which would to a certain extent be verifiable for other psychologists if a photograph of the dog had been taken in the act. From a similar study of a large number of such cases by trained observers, we would have the only scientific evidence obtainable as to whether dogs in general show that they can on occasion compare distances to a destination, and consciously choose what is thought to be the shorter. That is, the main evidence must always be from a complete record of expression, and that interpreted most cautiously.

Right here we wish to remark that Professor Morgan does not make clear to us how a perception of relations is confined in its ser-

viceability only to human 'descriptive intercommunication' (pp. 239, 243, 293). This assumption is quite too readily made, and used quite too much in an *a priori* fashion. In fact we may ask if perception of relations does not arise at first, not for communication, but by its immediate serviceability to the individual. Thus in the case of the dog and rabbit a definite understanding of space relations accomplishes more quickly and surely the catching of the rabbit; the dog profits by it quite as obviously as the human hunter who plans a short cut to head off a rabbit.

Again in explaining the apparent perceptions, *e. g.*, of distance, by animals, Professor Morgan insists that the relations, if perceived at all, are not focally but only marginally perceived, to use his optical terms. But this theory, which is fundamental with him, that consciousness in the development of its forms is first marginal merely, a side part in the total body of consciousness, and only gradually becomes focal or central, as in man perceiving a relation, seems quite contrary to the first assumption of evolutionary psychology, namely, that new modes originate in severest effort, and are thus in all their earlier developments preëminently focal. But when a body of consciousness, *i. e.*, a mind, is once formed and becomes hereditary, then much that has been focal in the long past becomes marginal. Thus vision in its origin was certainly not focal marginal, but a single focal point, and the highly developed vision that holds a considerable field of vision outside the single focus is really reflex of myriad ancestral focalizings. Hence, what is marginal to my vision is not the pin head on the cushion to my left, but the cushion itself, which is of such a size as to have been attentively perceived by numberless ancestral generations; but if for thousands of years my ancestors had exercised themselves in looking at single pin heads, I have no doubt that the pin head would be as plainly marginal as the cushion. So also we conceive that the present order of evolution indicates that the perception of relation was first realized, but faintly to be sure, in an intense focalizing effort. It is certainly a misuse of terms to make focal equal all clear and distinct consciousness; that which we are straining the eye to see is often far more faint than the marginal. We should say then that the development of perception was, like all other consciousness, from dim focal states to clear focal states, and then to marginal states.

The over use of hypothesis, and that often doubtful hypothesis, mars much of Professor Morgan's writing on animal psychology, but when he makes a survey of all the facts, his interpretations are in general just. However, since we are on negative criticism, let us

note an instance of experiment where the interpretation is very obviously defective; I refer to the throwing red currants to his chicks (p. 298). Though he calls this a 'parable,' I understand it has basis in fact. The chicks, seeing the strange objects, utter the 'what sound,' or note of interrogation. But having gained experience of taste of currents, what kind of sound will they utter upon coming upon them again? Mr. Morgan says, if they could attain to make 'currant sound,' this would mean absolutely nothing to the chick who had never experienced currants. 'It is a sound indicative of certain experiences that it has never had,' and hence 'of no indicative value,' and hence as 'value' or serviceability is the *rationale* of existence of psychosis and its expression there being no *rationale* here, the existence of a real 'what sound' and responsive answer may be denied.

To this we must suggest that while there is no 'currant sound' (Mr. Morgan here really falls into the language fallacy he elsewhere so justly condemns), there may be a sound indicative of edible object. Suppose a group of chicks before some currants, piping the interrogative, and the mother hen comes along with wide experience of those things we class and call currants, will she not give at once the food note? Just as when I come upon a comrade eating some strange thing, and to my interrogation he grunts 'yum! yum!,' and I know it is an edible. I take it that the food signal, which is certainly widely developed and widely useful among animals, is really no more than a kind of 'yum! yum!,' or a wholly indefinite pleasure sign of the edible. Every chick has food experience and so can appreciate food signal, though currant signal, if possible, would be of no use. I may suggest further with reference to the nature of the psychosis, of chick which has experiences of black caterpillar and learns to let it alone (p. 301) or red currant and learns to appropriate it, that we are not confined to the alternate hypothesis of mechanical association and full formal reason. Suppose a chick has seen and swallowed several currants with satisfaction, and running a little farther sees another currant, what is then its real psychosis! I am inclined to think, if the chick is yet in the active investigating stage, *i. e.*, beyond where it pecks at everything, but is becoming actively discriminatory, we may interpret the psychosis as identification. It recognizes that red object before it as the very identical object it has just experienced with such lively satisfaction, and it eats it (again). There is for it simply the single identical thing constantly reappearing, and so no things or classes of things. It eats its cake, and has it too. To the chick there is one worm and one only, which to its great joy it is continually re-

finding. (On this phase of psychosis I have made some fuller remarks in 'Evolutionary Psychology of Feeling,' p. 2, 85 ff.) And this does not deny that the identifying act is largely instinctive, *i. e.*, impelled by heredity force. Most of the apparently intelligent activities of young animals are doubtless fully three parts instinct to one part individual intelligence.

We have emphasized the need for an unexciting method in animal psychology, and for one which shall make it a point to secure in every case all the facts of expression. But while we can judge with some aptness what psychosis our fellow men are experiencing, we are so distantly related to most animals that their mentalities must be quite diverse in tone, degree, and quantity from our own, the only basis for our judgment. Hence animal psychology should begin with those animals most akin to us, as the simians, and the psychologist should seek the most constant and intimate acquaintance with his charges, should practically live with monkeys till he becomes thoroughly conversant with their modes of expression. Further, in observing and experimenting for intelligence, mature animals should be chosen, those who have run through all the stages of hereditary mind. But they should not be old, the most favorable age being for a year or two following full development, when there is plasticity, and yet recapitulation is fully done, all the forms of mere instinctive adaptability being fulfilled. Professor Morgan's experiments were mainly if not entirely with young animals, as chicks and pups.

Further, the motive to the creative activity of real intelligence must be an adequate and favorable one. The hunger method is perhaps the most efficient in nature, and it is proverbial that hunger sharpens wit. In nature most of the progressive intelligence has been achieved by animals confronted by new circumstances in their search for food. Hence the test which would most likely to give definite positive results for initiative intelligence of animals would be to confine a just matured chimpanzee in a cage, and, having well starved him, put food near by under conditions which neither his nor his ancestors could have experienced, but conditions which might be overcome by some simple perception of relations and application thereof. This gives opportunity for cool deliberative action; and the monkey should be carefully studied and photographed and phonographed throughout the whole test. Of course such tests, if pursued too far, might well call for the interference of the Humane Society. In fact to reproduce the conditions which under natural selection stimulate creative intelligence may always mean cruelty. The successful mind has generally made its achieve-

ment at the critical life and death point. Yet it may be that with monkeys seeking food or liberty, there can be provided sufficient incitement to obtain positive results without cruelty.

We must add that wild animals, as being distinctly more alert than domesticated, are the most desirable subjects in seeking positive results as to the intellectual powers of animals. Adaptiveness not being forced by the conditions of existence on tame animals, they become little more than machines. Compare thus the wild sheep and the tame. The dog in being 'well trained' to be routinely obedient to his master is made a mechanical slave; he loses very largely that free initiative and strong intelligent individuality and independence which was his, when, in state of nature, he was his own master and had to provide for himself or starve. The dogs at our bench shows are mostly very stupid and helpless beasts. As domesticated animals are bred in the main not for psychical but for physical points, man has as a whole degraded and brutalized the brute. Certain passive and emotional states are indeed generally favored by man, as lack of temper, but it is only exceptionally, as in the collie dog, that the animal is bred distinctly for intellectual qualities. Even the collie fanciers look chiefly to the coat of the animal, form of the head, tail, etc. This tendency with breeders is, in truth, much to be regretted both from a sentimental and scientific point of view. If selection and breeding were definitely carried on with dogs wholly in psychical lines for a number of generations, we should have some far more interesting companions than the present prize beasts, and some far more suggestive material for the comparative psychologist.

While our remarks have been directed to some questionable points in Professor Morgan's animal psychology, we are thoroughly convinced that he has accomplished much that is suggestive, and that his basis of introspection is the only true basis. His caution is also admirable, but we do not think the law of parsimony is positive proof, as he seems to urge. Thus, as applied in the dog-and-drain case, the question is not what might be, but what are the actual psychic facts as interpreted from actual expression. The golden rule of science is that theory is good only as leading to facts and facts only as leading to theory, but animal psychology is yet far from attaining this full correlation.

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RECOGNITION.

In Miss Mary W. Calkins' full, able and, if I may be permitted to say so, sympathetic review of my two articles on the 'Recognition Theory' of Perception of Höffding, Spencer, Ward and others, and on Recognition, there is an important misunderstanding. By the omission of two words, indicated of course by asterisks, I am understood and quoted as saying that there is in recognition no identification of the past with the present. It is, I believe, a misunderstanding which has very important bearings on a right understanding of recognition.

The remark I made was: "There is in recognition no 'identification of the past impression with the present one.'" The 'past impression' I hold to be gone forever, to be no longer existent and hence not a participant in any comparison or identification which may possibly take place in recognition. Former theories of recognition have, I believe, misrepresented the facts by asserting that in recognition and memory the 'former impression' is present and that it is known as past or known again. Then by some special *actus* of the 'mind' this 'past impression' is compared and identified with some 'present' object and we know that this object is known again. Again, there is a further *actus* supposed in the 'mind's' capacity of preserving, retaining and bringing to light again the former impression or object. The 'Retentive Faculty' is still abroad if not openly, still covertly.

Now I hold that in recognition it is not the old or former impression or 'way in which consciousness looks at a thing' (call it what name you will) which is present. It is gone and gone forever. It is the *object* (I speak simply of 'things' as they appear in consciousness and with no metaphysical theory in view) which is known as past or known again and not the former impression. Upon the basis of certain characteristics, as I explain later, I classify some objects as 'past,' some 'present' and others as 'future.' The former impression is not present, for it no longer exists.

Pastness or the known-again-ness of objects cannot therefore be explained, as is usually done, by a comparison and identification of the object of perception with the 'past impression.' Even if it did now exist the comparison would be between two objects, and whence then the pastness? How in the meanwhile has the object, as then perceived, become 'past'? If simply resurrected it ought to be the same as before. Furthermore, it is a definition in a circle to explain pastness by bringing in this 'past impression' as an explanatory term of pastness. That is the point to be more precisely elucidated by the

definition. Why is an object regarded as past or known again? Is that 'past impression' which is used to explain pastness or known-againness to be explained in its turn again by a comparison or identification with other 'past impressions' and so on *ad infinitum?* We know of no such process surely in consciousness. Or again, if an object is known as past in itself, *i. e.*, inherently or ultimately, there appears to be no need of a process of comparison and identification and if such be the case, how is it that we regard the same object or event at one time as past, and at another as present, or future?

This leads to the second point. Miss Calkins believes 'the central error of the theory' to be 'the assertion that recognition does not imply identification or comparison.' It is further remarked that 'immediate recognition does, nevertheless, include comparison with the past experience of the subject, only the comparison is wavering and restless, and the identification is incomplete.' As above stated, I did not assert that identification and comparison in recognition were impossibilities or absent in the process. I merely said, "there is in recognition no 'identification of the *past impression* with the present one.'" (p. 269.) The process of comparison and identification may enter into some cases of memory and recognition, but is not an integral and necessary part of every case of recognition. After an idea-object (centrally excited) has arisen it may be classified upon certain characteristics as 'past;' then the perceptual object (peripherally excited) may be compared and possibly identified with the primary object. The perceptual object may then be classified as known again. In such a case comparison may be present, but it was not necessary for the classification of the idea-object as past. So it is with most cases of sudden recognition or of strange familiarity where we could not possibly have seen the object in question beforehand. Some characteristic, usually appertaining to objects we call past, associates itself unwontedly with the object perceived and the classification naturally ensues. No 'past impression' or even idea-object is apparently present or necessary for the recognition in question. In the classification of an idea-object as past, is it necessary that still another idea-object (past or former, or what you will) should be present, compared and identified and so on *ad libitum?*

Objects as they appear in consciousness are in themselves neither past nor present. So-called idea-presentations stand, in this respect, equally on a par with the sense-presentations. Comparison or identification of a sense-object A with the centrally excited or idea-object *a* will give us no recognition, simply object A or *a*. The pastness or

known againness of either has still to be ascertained. Equally so, and this is a point little regarded, does the *presentness*, the *nowness* of certain objects require to be explained.

An object may be regarded at one time as past and at another as present. Why should it be thus classified differently at different times? Upon a consideration of these points I was led to note the characteristics of the objects (and their possible accompaniments) in each case. It then became evident that when objects were possessed of certain characteristics as *e. g.*, lack of freshness and vividness, absence of details, unsteady, easily changeable localization, lack of persistency, air of freedom, absence of certain muscle, joint and other sensations, the sudden introduction into consciousness of an object by association of ideas, which object does not in the case in question properly belong to the object perceived, the great rapidity and often surprising ease and quickness of the act of perceiving often accompanied by a second idea-presentation of the same object immediately following, or often a feeling of pleasure upon perception of an object, say a stranger in the street, when the cause of the pleasure is unknown, etc., then, I say, we have a consciousness of these characteristics and classify these objects as past or known-again. If, on the other hand, they possess vividness, full details, persistence or obstinacy of spatialization, persistency in abiding under certain conditions, etc., then we have a consciousness of these characteristics and put them in the other great class of objects which we name *present*. The former we call memories, the latter perceptions. Thus it happens that upon an object centrally excited, possessing great vividness, persistency, etc., arising, there may ensue the classification of it as belonging to 'objects present;' later it proves to be an hallucination. It may also be added that objects may be possessed of these characteristics, but they may not be noticed and there may be no ensuing classification. In such a case, there is simply what I may term 'object consciousness' passing on to another 'object consciousness.' Neither the characteristics nor the classification, taken alone, make up recognition, but both together. Moreover the characteristics may be variable, now one, now many, now this, now that; the classification into either present or past objects remains, however, the same. The characteristics are, however, obviously not the same for each great group or class.

It is thus clear that I do not, as Miss Calkins affirms, 'treat the past as the known-again-with-its-associates,' nor do I exclude comparison or identification from all cases of recognition. In my own experience in the majority of those cases of strange familiarity which are

noted by so many, the process appears to me to be in most cases a surprising acceleration or ease in perception of the object which is undoubtedly hitherto unknown by me, and immediately following thereupon a second presentation of the same object. Now this acceleration and this consequent easy presentation of an object frequently given in experience, are characteristics upon which I base the immediately following classification as known again or past. There may be thus present comparison and classification, but it is evident from some of the other characteristics that they are not necessarily always present. It is said "when a face 'seems familiar,' I am eagerly comparing it with faces I have already seen, trying to identify the present with the past." It is quite obvious in such cases, however, that the comparison and identification comes *after* the strange feeling of familiarity which may be based on other characteristics than the presence of accompanying idea-presentations. Moreover as above stated, it does not seem correct to use the explanatory phrases 'past experience,' 'identifying the present with the past' etc., in explaining pastness.

ARTHUR ALLIN.

The points of disagreement between Dr. Allin and myself seem to me to be mainly metaphysical, and should perhaps have been untouched in my notice of his intentionally psychological articles. As I have there said, the "definition of the recognized as the 'known again' is psychologically quite satisfactory, for psychology avowedly adopts the matter-of-fact standpoint," that is, psychology deals with *facts* of consciousness, or relatively isolated, single realities, immediate and temporally located.[1] Now these facts of consciousness or impressions as Dr. Allin might call them, never recur and never rise from a buried past into a present. Under these circumstances the difficulty is to show why we do actually have an experience of what we call identity; why, in spite of the evanescence of the facts or events of consciousness, we do predicate sameness. The solution to this problem seems to me to be suggested by the following line of thought: besides the factual sort of consciousness, the series of conscious states which truly does form the proper object of psychological investigation, I believe myself to possess, actually and immediately, another sort of experience which is what I mean by the term 'self-consciousness;' and it is the characteristic of this sort of experience to be non-temporal and incapable of being split up into facts.

[1] *Cf.* F. H. Bradley, *Appearance and Reality*, p. 317, for a similar definition.

Now this self-consciousness evidently conflicts with the psychological or fact-way of regarding consciousness precisely where questions of time are involved, but the self-consciousness is the immediate experience, while the 'facts' are really artificial abstractions, necessarily hypothesized for the scientific study of consciousness, yet in no sense concrete realities. Moreover, in defining a 'fact' as temporarily related, it is easy to assume the fundamental validity of time distinctions, whereas from the strictly psychological point of view they are mere conscious elements. To say, therefore, 'the past impression is gone forever'—a statement to which I cordially subscribe—means: "Assuming, by use of the word 'impression,' the temporal way of regarding consciousness as a series of 'events,' then it follows that one 'event' is not temporally identical with another." This does not, however, affect the reality of the experience of 'identifying,' which is really a transcendance, not a comparison, of past and present.

Dr. Allin's close parallel of the 'known again' with the sensation[1] seems to me also to threaten the obliteration of an obvious distinction among 'facts of consciousness.' On the one hand, we have the admitted sensations, the 'red,' the 'shrill,' the 'hard'—what Dr. James calls 'substantive parts'[2] of consciousness. It is their characteristic to be independent, to stand alone as it were, or (adopting Dr. James's figure) to provide perchings and landing places to thought. Besides these, however, there are the 'transitive parts'[2] or 'fringes,'[3] the relations or links, themselves facts of consciousness, and facts only, from the psychological standpoint, yet lacking the independence and self-sufficiency of the substantive elements. Such 'transitive parts' are 'sameness,' 'agreeableness' and 'disagreeableness'—not to mention others which might lead us far afield; these seem to me to be distinguished from the substantive elements or sensations, just in this, that they inevitably suggest the immediate self-consciousness which, however, unlike the 'facts,' is untemporal; the puzzle of assumed identity and temporal diversity is thus an opposition of the two points of view.

Though I agree, therefore, with Dr. Allin, in the belief that, psychologically speaking, present and past are simple elements of consciousness, I nevertheless do not regard his analysis of these contents as psychologically sufficient. He seems to me himself to suggest the inadequacy by the statement, in the preceding 'discussion,' that recognition requires both the enumerated 'characteristics' (lack of vivid-

[1] *Amer. Journ. of Psy., op. cit.*, p. 267.
[2] *Principles of Psychology*, I., 243, *et. alt.*
[3] *Ib.*, p. 258.

ness, rapidity and the rest), and an 'ensuing classification.' Since these characteristics 'may be variable, now one, now many, now this, now that,' it seems to me likely that they are mere accompaniments, not constituents, of pastness which apparently remains virtually 'what-is-classified-as-past.'

According to my view, which I can here barely suggest, the consciousness of time-distinctions is relatively late, and is one form of consciousness of multiplicity. It presupposes self-consciousness, for the past is primarily one's own past, and only later do mere objects of imagination unconnected with one's own experience, like the reforms of kleisthenes or the battle of Waterloo, become also 'past.' The essence of temporal multiplicity is, however, the consciousness of necessary connection. The 'past' is the 'irrevocable,' or 'irreversible;' the future is the 'supposedly reversible or unconnected;' the present is a later distinction and is negatively defined with reference to the other two. Further we surely can not go on any pretext of keeping within psychological bounds; it may indeed be questioned whether we have not already transgressed these in attempting any account of 'pastness.'

Two points in Dr. Allin's criticism of my review may be briefly mentioned. The word 'past' which occurs in the statement, quoted by Dr. Allin, about 'immediate recognition,' he regards as a case of 'definition in a circle.' The word was not used, however, as an explanation, but as a partial analysis—though a superficial one—of the 'known again,' which is not in my opinion equivalent with the 'past.' The sentence in which this word occurs is properly criticised, since it treats the subject rather popularly and inexactly, but I still hold, with reference to the main point at issue, that mediate and immediate recognition differ only in degree.

It remains to question two of Dr. Allin's explanations. He does not seem to me greatly to advance the discussion by insisting that recognition is of 'objects' not of 'impressions,' since he does not clearly define the former word. If he means frankly 'common-sense object,' then, indeed, he has an honest psychology, but it merely substitutes an every-day philosophical assumption for a more subtle one. I object also to the recourse (as by the expression, 'object centrally excited') to cortical conditions as explanation of psychological phenomena, for here again one has on one's hands a whole series of metaphysical assumptions—dualism, physical causality and so on—intermingling with one's psychology.

MARY WHITON CALKINS.

WELLESLEY COLLEGE, May, 1896.

THE COMMUNITY OF IDEAS OF MEN AND WOMEN.

In following the discussion between Dr. Jastrow and Miss Calkins on the Community of Ideas of Men and Women, I have been struck most forcibly by their not distinguishing the two problems which Miss Calkins finally states at the end of her article in the PSYCHOLOGICAL REVIEW for July. These problems, put in terms of the two principal points at issue between Dr. Jastrow and Miss Calkins, are:

1. Do women *naturally* tend more to repetition and to the use of concrete terms than men?

2. Do women tend more than men to repetition and the use of concrete terms, *on account of education and social traditions?*

The first problem deals with genuine mental differences of sex; the second with differences due to association, not therefore differences of sex, but differences which will change with changes in education and psychical environment.

Which problem are Dr. Jastrow and Miss Calkins discussing? I find it nowhere explicitly stated, but the fact that the two sexes as such are experimented upon to find their differences leads me to suppose that the first problem is the one under consideration. From that point of view it seems to me that some criticisms may fairly be made upon their method of collecting data.

If the problem is to determine inherent psychical sex differences the first essential to scientific experiment is to eliminate, as far as possible, or to allow for differences due to habit. This can be done to a large extent: (a) by selecting men and women who have had from childhood essentially the same physical and psychical training; (b) by a detailed account of the differences in training which do exist; (c) by a large number of cases chosen from different professions and different social strata; (d) by making a record, at the time when the lists are written, of the studies which the subjects are pursuing and of their occupations outside of their university work; (e) by having the subjects under the same conditions when making out the lists. I am even inclined to say that they should be given the same word to start with. After the lists are written it would also be an advantage to have the subjects write out the association between the words, in order to help in the classification. In this way, probably some apparently abstract terms would turn out to be concrete in meaning.

As nearly as I can judge from the articles, none of these conditions were observed by Dr. Jastrow and Miss Calkins. No measures were taken to eliminate or to allow for the influence of habitual associations.

To my mind, therefore, the experiments simply resolve themselves into a further illustration of the very well-known fact that habit determines the association of ideas—a fact which it is entirely unnecessary to prove and which is quite as strikingly illustrated by the different associations among men of different professions as by those between men and women.

It is probable that the most striking differences between the Wellesley lists of '94 and '96, and between the Wellesley and Wisconsin lists, might be entirely explained by a few inquiries into the studies taken by the different students at the time when the lists were made. For instance, the lists of abstract terms stand thus:

Wis. Men.	Wis. Women.	Wellesley, '96.	Wellesley, '94.
131.	97.	101.	280.

We are told that in the Wellesley list for '94 one paper alone contained fifty abstract terms out of a hundred. Until it is positively disproved, we could hardly escape the inference that this subject, for some unusual reason, had been much occupied in abstract thought. May it not be true that inquiries about the Wisconsin men and women would give the same kind of explanation? Take, for instance, the lists on the animal kingdom:

Wis. Men.	Wis. Women.	Wellesley, '96.	Wellesley, '94.
254.	178.	146.	223.

The discrepancies here might be doubly explained. Some of the subjects might have been taking zoölogy or biology, in the first place. In the second place, the care of animals always falls to the boys, very seldom to the girls.

So I might go through with the entire list, but these suffice to show my point.

We may grant and declare that women's associations differ from men's, because their habits of life are different. We may admit the certainty of there being some psychical differences between men and women on account of the physical differences of sex; but generalizations as to inherent psychical sex differences which are made on the basis of variations due to *individual habits* can have no validity.

It may be objected that the very fact that women do follow certain occupations to the almost entire exclusion of men, and *vice versa*, proves certain *particular* differences. This may be so, but it can not be demonstrated until men and women are not only nominally but actually free to enter any profession. At present some occupations are

both nominally and actually, and many others are actually, closed to women, especially to married women. The same is true of men, although to a much smaller degree. In view of the recent enlargements of 'woman's sphere,' he would be a bold mathematician who would attempt to give its radius. The real tendencies of women can not be known until they are free to choose, any more than those of a tied-up dog can be.

Such generalizations as those of Dr. Jastrow and Miss Calkins, serve only to confuse the point at issue. Whether the experiments were to prove inborn psychical variations between men and women or differences in association due to differences in the modes of life, they fail equally because they do not consider the effect of habit; and in the latter case they have even less *raison d'être* than in the former, because the fact is already generally admitted.

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PSYCHOLOGICAL LITERATURE.

RECENT FRENCH WORKS.

Du fondement de l'induction, suivi de psychologie et métaphysique. T. LACHELIER. Paris, Alcan, 1896. Pp. 176.

This little book is a republication of a doctor's thesis which dates from 1871 and has been long out of print. Lachelier undertakes the investigation of the principle on which is based the operation by which we pass from knowledge of facts to knowledge of laws, or, in other words, by which we add to perceived facts the elements of universality and necessity, which characterize laws.

According to the author, "there are only three ways to account for principles, because there are only three ways to conceive of reality and the act by which the mind enters into relation with it. In the first place, one could admit, following Hume and Mill, that all knowledge is sensation and that principles are only the most general results of experience. Secondly, one might assume, following the school of Cousin, that these phenomena are but the manifestation of a world of reality inaccessible to our senses, and in this case the chief source of knowledge would be a kind of intellectual intuition, which discloses the nature of these realities and the action that they exert on the sensible world. Finally, according to a third hypothesis, that of Kant, our highest knowledge is neither a sensation nor an intellectual intuition, but an operation by which thought perceives immediately its own nature and its relation to phenomena."

Without stopping to discuss Cousin's theory, we may adopt on this point Lachelier's conclusion: "Substances and causes are only a desideratum of science, a name given to the unknown basis that maintains the order of the world, the statement of a problem transformed to a solution by a verbal artifice."

But attention should be called to the interpretation, quite incorrect in my opinion, which Lachelier gives to Mill's theory, for I think there is no radical difference between the points of view of Mill and of Kant. Lachelier has fallen into the error of believing that Mill takes sensation and experience, not as the point of departure of knowledge, but as themselves constituting knowledge; this is evi-

dently what he regards as empiricism, and I do not say that Mill is not responsible for the mistake, as his language is often equivocal. But let us take first Mill's argument and see what Lachelier answers. It appears to him that Mill is in difficulties between the needs of science and the logic of empiricism. But I feel sure that Mill's argument does not beg the question when his meaning is thoroughly understood. This is summarized as follows: "The spontaneous induction that first suggested to men the regularity of the most common phenomena inspired them with only moderate confidence, but their confidence gradually increased as experience confirmed the results of their early inductions, and each fact that confirmed a special law spoke in favor of the law of causality, which thus collected for itself as much favorable testimony as all the others taken together. It is consequently not surprising that this law became finally invested with absolute certainty, while the others only attained a greater or less degree of probability," (p. 20). Lachelier does not accept this argument, but he does not indicate clearly his reasons. The only objection to be made to Mill, in my opinion, is that he has not been sufficiently explicit and has not said that the principle of induction is psychological, that it depends on our mental constitution and that all reasoning, even deduction, is based on the mechanism of our ideas and representations.

Let us now examine Lachelier's arguments, which are curious; the chief ones are as follows: What is spontaneous induction, and what place does it occupy in a system in which experience is regarded as the only source of knowledge? Is it the same thing to observe the occurrence of phenomena and to conclude that the same phenomena will recur under the same conditions? The author amplifies these curious statements in another place in the same book to refute the view of Royer-Collard, according to which the belief in the stability of natural laws depends on our own nature. "It is difficult to imagine a more complete confusion of ideas. Our nature is not able to teach us *a priori* regarding a fact of experience, but beyond experience and facts there is nothing but the truths of reason, which do not admit of contradiction. A judgment which is not empirical and yet is not necessary, is an absurdity which has no place in human intelligence." It would take too much space to answer point by point, but let us note some of the arguments. Lechelier admits a few lines further on that we are able to foresee certain events as probable. He admits that this foresight is accompanied by a strong and even irresistible tendency of the imagination, but then expressions of contempt

follow, . . . "to seek the secret of the future in what is only the vague image of the past is to undertake to guess during a dream what will happen when we are awake." What a curious idea to compare with a dream the regular course of mental life! It is much to be wished that this question of the basis of induction, which is in the first instance psychological, should be taken up by psychologists and studied by means of exact observations.

Le mouvement idéalist et la réaction contre la science positive. A. Fouillée. Paris, Alcan, 1896. Pp. 351.

This work, from the fluent pen of one of the best known of French philosophers, aims to bring to a focus the discussion on the fallibility of science so brilliantly opened by M. Brunetière. It will be remembered that M. Brunetière, in reviewing the questions which modern science is unable to answer, confined himself chiefly to the physical and natural sciences. It is in the name of the moral sciences that Fouillée speaks, and he thus changes somewhat the question at issue. He treats as the principal adversaries of science, or, to speak more exactly, as the teachings which limit the field of science, the agnosticism of Spencer, the idealism of Kant, and the philosophy of contingency, represented by Renouvier and Boutroux. These are the teachings he discusses and seeks to refute. It does not seem to the present writer that the intellectual unrest and the reaction against science which have arisen in recent years have anything in common with the discussion of these philosophical problems, and I think it would have been preferable to have written a natural history of the moral anarchy of our society, its causes and consequences, remaining as far as possible within the limits of observed facts.

The work has an appendix containing four short papers, (1) 'Adolophe Franck and the Philosophic Movement of the Past Fifteen Years,' (2) 'Descartes and Contemporary Teachings,' (3) 'Philosophic Instruction and the French Democracy,' (4) 'Philosophy in Examinations (*les concours d'agrégation*).' The author regrets the exaggerated place given to metaphysics and the history of philosophy.

Les principes du positivisme contemporain, exposé et critiqué. T. Halleux. Paris, Alcan, 1896. Pp. 351.

This little book written from the point of view of catholicism and inspired by M. Mercier, of Louvain, contains many ready-made formulas and purely verbal arguments, such as are usual in a catechism, The principal objection made to positivism contains a great deal of

truth. It is that the positivists are mistaken in holding that all experimental knowledge can be reduced to the consciousness of a subjective being; in the most minute and exact observations the mind always controls the senses.

Les types Intellectuels: Esprits logiques et esprits faux. Fr. Paulhan. Paris, Alcan, 1896. Pp. 362.

In this book, with this curious and suggestive title, the author proposes a classification of intellectual types. The study is a continuation of his previous work, *Sur les charactères*, the two works being the development and application of the author's peculiar theories regarding systematic association. It may be briefly called to mind that systematic association consists in the property that all kinds of psychological elements possess of associating themselves together to form syntheses, not in obeying the laws of resemblance, of contrast and of contiguity, which are secondary laws, but in realizing a law of teleology. In intellectual phenomena systematic association takes the form of knowledge, whence a division of intellectual types into such as are logical and such as are illogical.

Logical minds are of various kinds: *well-balanced*, in which systematic association follows without effort from innate tendencies; *thinkers*, in whom equilibrium is sought after and attained with effort; *extremists*, in whom equilibrium is obtained by the subordination of the intelligence to certain elements; *specialists*, in whom systematization takes place only in a small field. Then there are the intellectual types dominated by phenomena of conflict, of inhibition and of contrast, the combatants, the critics, the dreamers, the sceptics, etc. The exaggeration of association by contiguity gives a limited and halting memory; the excess of association by resemblance, in the case of words, gives rise to rhyming poets; in the case of ideas, to the abuse of metaphor.

The author also subdivides illogical types. He distinguishes minds illogical through the excessive predominance of leading ideas; those illogical through conflicting adaptations; then, finally, those naturally incoherent. This last type is confined to children and hysterics.

All these distinctions are interesting, but we should not forget that the study of intellectual types should be made by observations of individuals, rather than by a treatise written in the library.

De l'aphasie sensorielle. CH. MIRALLIÉ. Paris, Steinheil, 1896. Pp. 220.

This book is a thesis for the M. D. degree, in which the author chiefly presents the views of Dejerine, his teacher. It is well known that Dejerine, the eminent professor at the Saltpétrière, has made numerous researches on aphasia which have opened a new phase of the subject. Charcot's celebrated scheme of the four images and the four centers has received a serious attack; briefly, the chief points brought out are as follows:

(1) Charcot held, following Hartley, that we can use in inner speech four kinds of images, visual, auditory, articulatory motor and graphic motor, and that consequently language depends on the use of four distinct nervous centers, and, further, that each individual uses preferably a certain kind of images, some being visualizers, some being audiles, etc. Dejerine holds, on the other hand, that this distinction of mental types is not founded in fact, but results in a confusion between memory for words and memory for things. As far as memory for objects is concerned, it is quite true that there is a visual memory, an auditory memory and a motor memory, and that some kinds of memory may be more developed in the case of certain individuals and serve to characterize them; but in regard to words and thinking in words matters are quite different. In studying inner speech we must return to the point of view of Egger. We are all auditory, or, more exactly, auditory-motor, and those who 'read the words of their thought' or 'write them' are extremely rare exceptions. The whole discussion should be read in Mirallié's monograph, which, though presented in a somewhat schematic form, is highly instructive.

(2) The second part of Charcot's work, which has been refuted by Dejerine, is the explanation of agraphia. According to Charcot the act of writing depends on the calling up of graphic images, and agraphia is explained by the loss of graphic memory. Dejerine has brought forward many cases to show that the process of writing is entirely different from this; we use a visual copy, and it is the visual image which is lost in agraphia and prevents writing. This is proved by the fact that those suffering from agraphia can write when they copy a model placed before their eyes, whereas they are unable to make words from blocks containing the letters.

Mirallié's book includes further an exposition of the two forms of verbal blindness distinguished by Dejerine, and a plea in favor of sensorial aphasia which Wernicke defined, but which Kussmaul and Charcot have denied. There are in the book numerous anatomical

drawings and specimens of handwriting, clinical observations given in detail, and a very complete bibliography of aphasia. It is impossible to recommend too highly the reading of this book to those who wish to understand the most recent studies of aphasia, a subject of the greatest possible interest to the psychologist.

A. BINET.

PARIS.

Der Kampf um einen geistigen Lebensinhalt: neue Grundlegung einer Weltanschauung. RUDOLF EUCKEN. Leipzig, Veit & Comp, 1896. Pp. viii+400.

The significance of this work is indicated by the chief words in the title. The present age is held to be one in which man is threatened with the loss of all sure foundation for the life of the spirit, or even actually dispossessed of a basis and a content for his spiritual existence. Thus our time is one of conflict. First of all, conflict between the opposing tendencies of thought, which distract us no more surely when considered in their manifold variety than they fail to satisfy the mind if taken in the form of the movements—*e. g.*, naturalism, idealism—that most have gained the suffrages of the modern world. Hence begins a deeper struggle, or rather the conflicting systems of the day include an element of which the leaders are often but dimly conscious, a contest for the realization of the life of spirit and for the satisfaction of the needs native to man in virtue of his relation to the universal spirit or reason of the world. This battle and this yearning unrest, moreover, are real, however much they may be denied or disguised by the complacent naturalism of the time, by our shallow culture and our lifeless art, by a social utilitarianism at bottom essentially selfish, or by a weakened church which fails to accomplish its high mission because of its insistence on the outworn traditions of the past. The age, therefore, must be summoned to continue its warfare; only with an adequate comprehension of the issues at stake and of the true objective point of the conflict. For the *Geisteswelt* and the *Geistesleben* are fundamental realities, not mere imaginings crystallized into words. Yet their reality is to be understood in a sense other than that which is commonly associated with the term; they exist not as fixed and finished products, but ever depend on the work of free creative activity. Especially for us, a constantly repeated deed, which implies freedom and is in its nature essentially ethical, is necessary, if we are to realize the spiritual life-process in ourselves. In this way only is the generation of a true spiritual actuality possible; and possible, the overcom-

ing of the contradictions whose existence and whose power it is idle to attempt to ignore. *Das Ansichwahre und Ansichgute Platos, es wird zu einer lebendigen Wirklichkeit für uns nur in Verbindung mit jener Selbstthätigkeit Fichtes* (p. 33).

In tracing the conditions and the course of spiritual advance through conflict Professor Eucken divides his work into two main parts. The aim of the first or *Aufsteigender Teil* is the defense and elaboration of his chief thesis; that of the second or *Absteigender Teil*, the application of his results to the concrete conditions and institutions of to-day. Part I. subdivides again into three discussions of as many stages of the movement: A, *Der Kampf um die Selbständigkeit des Geisteslebens;* B, *Der Kampf um den Charakter des Geisteslebens;* C, *Der Kampf um die Weltmacht des Geisteslebens.* In these, besides the principles of the spiritual life already noted, two others may be mentioned as essential to the author's doctrine and constantly kept by him in view: the existence of a universal spirit or reason, to whom man is fundamentally related and whose ultimate victory is absolutely sure; and the development of the new world of spiritual activity with ever-increasing richness and complexity as one by one the various forms of opposition are overcome. In fact, though the contradictions and the conflict are painfully real, they contribute in their turn to the development of the spiritual life-process as it conquers the opposing forces by transforming and conserving them. Part II. brings the general view of the life of spirit thus gained into normative connection with the present status of affairs. Here, as throughout his treatise, Professor Eucken finds much to criticise in the organization and institutions of the age and sees hope only in the correction of present tendencies by resolute devotion to the spiritual ideals. This adherence, however, must not be partial, but inclusive. Even religion, with its clearest intimation of the world beyond, may exert a pernicious influence if it assume to be the whole of the spiritual process instead of finding its complements in morals, art and philosophy. It is only when all these several agencies, purified of their one-sided tendencies as well as of their direction to that which is empirical and lower, are combined into one collective movement that an age (or a man) can rise to the measure of its spiritual possibilities.

For the purposes of this REVIEW detailed criticism is not in place. It may be remarked, however, that Professor Eucken furnishes one more interesting proof of the dissatisfaction of earnest thinkers with the outcome of recent speculation. Happily his criticism is more temperate than that of many other judges of the age. With all his

directness of censure, his historical sense is too sure and his appreciation of modern culture too real for him to overwhelm us with an unrelieved jeremiad, even were his belief in the final triumph of spirit less absolute. Therefore his collateral discussions are often illuminating even for the adherents of principles which he rejects. And, as has been suggested by a critic of one of his earlier works in which among others, positions similar to those of the volume under review have been foreshadowed, his general view of the world and of the age may prove acceptable to many who can not fully share in his positive philosophical doctrine.

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ETHNOLOGY AND ANTHROPOLOGY.

Ethnology. A. H. KEANE, F. R. G. S. Cambridge Geographical Series. Cambridge University Press, 1896. Pp. xxx + 442.

One takes up with interest any professedly synthetic work on a subject as disordered in its material as anthropology or ethnology, and while any book with as ambitious a field as the title of the one before us would indicate must be almost immediately superseded it is worth while every now and then to pause and take our bearings. Of Mr. Keane's book, in the first place, it must be admitted that the title is misleading, although he partially guards himself by the definitions with which he very properly opens. Whatever may be said of other anthropological terms which are at present in such active dispute, 'ethnology' has come to be pretty generally regarded as including the comparative study of the varieties of man in their social aspects, and while the term 'ethnography' is, of course, a necessary one to denote the purely descriptive side of the subject it is always subsidiary to the larger, and an ethnology without an ethnography is an absurdity which apparently does not bother Mr. Keane in the least, since he hands over what we have all come to regard as among the main questions of ethnology to ethnography, and restricts his own work to a field which he divides into two parts, treating in the first place, under fundamental problems, such questions as the 'Physical Evolution of Man,' 'Mental Evolution of Man,' 'Antiquity of Man,' 'Specific Unity of Man,' and 'Varietal Diversity of Man,' and in the second part taking up the primary ethnical groups which he divides into four, 'homo Æthiopicus,' 'homo Mongolicus,' 'homo Americanus,' and 'homo Caucasicus.' If we accept this contracted field of ethnology, Mr. Keane's work is, on the

whole, well done. One cannot expect too much from a general work of small compass, yet serious exception must be taken to such chapters as that on 'Mental Evolution of Man,' which is most inadequate from the point of view of comparative psychology, the chapter being a short one of nine pages treating chiefly of craniology, and similarly to the one on 'Mental Criteria of Race,' which confines itself almost wholly to a discussion of language, a comparative feature of prime importance, of course, but in the light of such researches as those of Tylor, Bastian, Lippert, Steinmetz and others no longer to be regarded as the only field of comparative value.

Yet with all its shortcomings the book satisfies a genuine need, especially of the general public. Mr. Keane's reading is wide, his presentation of arguments fairly complete and the arrangement of material logical, and his book is temporarily at least perhaps the best resumé at hand of our knowledge in the limited field of which it treats, which is unfortunately rather faint praise.

The Child and Childhood in Folk-thought. A. F. CHAMBERLAIN, M. A., Ph. D. New York, Macmillan & Co., 1896. Pp. x+464.

Dr. Chamberlain has produced a very useful, painstaking and disappointing work. It is useful in the number of items he has collected from all sorts of comparatively inaccessible sources; it is disappointing in the almost total lack of logical arrangement of the facts. He has strung his beads of quotation upon a thread of thirty-three rather sentimentally headed chapters from which even the excellent index does not suffice to bring order, and the result is rather a concordance to the literature of the primitive child than a systematic treatise on the subject. His method, if there be one, is undiscoverable, and the book seems to fail to fulfill either of its possible aims, for it is an impossible work for the layman to read consecutively and emerge with any tangible results, and it is exasperating to the anthropologist who seeks material on any one of the really innumerable subjects connected with the position and treatment of the child among primitive people. The latter will with some difficulty find scattered through the book excerpts to his purpose introduced possibly by a gem from the pen of Henry Ward Beecher or Joaquin Miller, but a discussion by the author rarely. Take, for example, the chapter on 'Child-life and Education in General,' one of the best in the book, by the way, and such questions as the first moral training, first punishable offences, methods of punishment, etc., are hardly touched upon, much less discussed, not-

withstanding their immense significance. Facts as facts are always desirable however presented, but it seems a pity that one as fitted for the task as the author of this work should have failed so signally to utilize the extensive material he has recorded. Possibly this is reserved for further efforts. Let us hope that Dr. Chamberlain will see his way clear to bring future order out of present chaos. Of his book as it stands one can only say that as an example of industry it is remarkable; as science it is bad.

Die Anfänge der Kunst. ERNST GROSSE, Dr. Phil. Freiburg i. B. and Leipzig, J. C. B. Mohr, 1894. Pp. vii.+301.

This book is a little masterpiece. It is, so far as the writer is aware, the first attempt, certainly the first successful attempt, to establish a science of art, as distinguished from history and philosophy of art, upon a scientific basis by legitimate methods. The first task of any science is not practical utility but theoretical insight, and the first task of a science of art is not the application but the recognition of the laws which govern the life and development of art. This end is for the present an ideal, but an ideal in the struggle toward which the conformity of art phenomena to developmental law may at least be shown, even though the details of the laws themselves be not demonstrable, and it is as a pioneer in this field that Herr Grosse deserves the highest praise. He has grappled boldly with great obstacles, recognizes his failures, does not over-estimate his successes, and has finally 'blazed' a path which must be followed in the future and followed with most significant results. He recognizes two aspects to the task of describing and explaining the phenomena of art, an individual and a social, and turning his attention to the social confines his researches very wisely to the most fundamental and at the same time most neglected field, viz.: the primitive art of primitive peoples, and applies the method of comparative ethnology.

Without discussing the gaps and faults of that method as at present in use, we can turn directly to the author's treatment of his material. He follows the old division of the arts into arts of rest and arts of movement, quoting Fechner's description "dass die Künste der einen Art durch ruhende, die der anderen durch bewegte oder zeitlich ablaufende Formen zu gefallen streben; jene demgemäss ruhende Massen so umgestalten oder combiniren—diese solche körperliche Bewegungen oder zeitliche Aenderungen erzeugen das der Kunstzweck erfüllt wird." Taking up the arts of rest, commonly known as 'pictorial,' the author considers as the probable original form that of ornament,

and as the original object to be ornamented, the human body, which is, therefore, discussed first, followed by the decorations of utensils and weapons; thirdly, he treats of drawings, paintings and sculptures which do not primarily serve decorative ends but have an independent meaning. The transition from the arts of rest to the arts of movement is represented by the dance, to which especial attention is given as being of extreme sociological importance, and which leads naturally to the consideration of poetry, since among primitive peoples the dance and song are always associated, and finally, primitive music is discussed.

One may choose for particular notice the chapters on ornaments of the body or 'die Kosmetik,' and on the dance as being perhaps the most valuable and suggestive. Regarding the former, after a consideration of the various forms of clothing and ornament among primitive people, Grosse goes on to discuss their practical meaning. One can divide all primitive ornaments of the body into two classes, those tending to attract and those tending to affright, not that any given decoration will fall under one of these heads to the exclusion of the other; on the contrary, it is usually both, for among primitive men as in our own level of civilization what makes a man terrible to his enemies or to other men makes him attractive to women. Undoubtedly the first and strongest incentive to ornamentation of the body is the desire to please, and in savage life it is one of the most powerful and indispensable factors in sexual selection. At that level the men are much more given to decoration than the women, contrary to the condition of affairs among civilized people. They resemble the higher animals in this respect. It is the primitive man who is the suitor, just as it is the male animal who woos the female. A primitive old maid is a thing unknown; the woman is always sure of marriage, while the man must often obtain a wife against great obstacles, and often remain in a state of forced and hated bachelordom for years. This sexual value explains the fact that decoration of the body is often first begun after the rites of puberty which mark the entrance of the boy to man's estate. But the man is warrior as well as potential husband, and has therefore a double object in bedecking his person, and as has been said, most ornament serves this double purpose. Red is not only the color for festivals, but the color for war; the headdress of feathers which increases the height is assumed in the dance as well as in battle, and the scars on the breast of the Australian, which excite the admiration of the women, arouse the fear of the enemy. It is hard to find an exclusively repellent orna-

mentation. Only certain patterns of painting the body seem to serve that end alone. As badges of authority, class and rank, ornament is little used among primitive men, for such distinctions do not exist there as they do among us, yet even there are seen the beginnings from which have developed the uniforms, gowns and accoutrements of our own military, academic and other degrees.

Space does not permit a discussion of the questions involved in the forms and development of the savage dance, though its rôle is all important in savage life. The pleasure in active and rhythmical movement, the pleasure in imitation and the relief in the expression of pressing emotion are a sufficient explanation of the passion with which primitive man cultivates this art. The significance of the primitive dance is striking. It fulfills not only a sexual end, but to a greater degree even a social one. The uniting of a body of men under the influence of a single emotion as seen in war dances, the union of heterogeneous tribal elements in certain dances of peace, suggest sociological bearings of the highest importance, and the field is one richly deserving the attention of the ethnologist.

It is one of the merits of Grosse's book that it does not attempt too many conclusions from rather scanty material. One point at least becomes evident; primitive art in most of its phases does not serve primarily an æsthetic end; it is first of all practical, and the purely æsthetic result is, so to speak, a by product. In music alone as a rule does the æsthetic appear as the single end in view. For the rest of the numberless questions suggested one can only refer to the book itself.

Herr Grosse is entitled to the greatest credit for what is in the opinion of the writer the most important contribution to this subject in many years.

LIVINGSTON FARRAND.

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L'Année psychologique. 2e année, 1895. H. BEAUNIS and A. BINET. Paris, Alcan, 1896. Pp. 1010.

The second volume of the *Année* presents a decided advance over its predecessor. The plan adopted at the outset included three distinct parts: original articles, summaries of important books and articles appearing during the year, and an annual bibliography of all publications of interest to psychologists. The same general scheme is adhered to in the present volume, but we find several noticeable changes in details. A larger number of articles are summarized, and the summaries themselves are more in keeping, as regards length, with

the value and interest of the works. Under the head of original articles are included a number of 'general reviews,' which add greatly to the value of the *Année*. In the same section, too, the work of the independent contributors is now separated from the more specialized studies of the Paris Laboratory. The fact that the volume has increased in a year from six hundred pages to over a thousand is in itself an indication of the proportions which the enterprise has assumed.

To the outsider this rapid expansion cannot appear as an unmixed blessing. A volume of the present size is not over easy to handle; if the number of original contributions should be further increased (they are still comparatively few), it might actually become unwieldy. We may ask whether, after all, the plan laid down is not too complex to be carried out as a single undertaking. Examination shows that the volume includes two distinct lines of work, which might readily be separated. The first is a general résumé and bibliography of the past year's work in psychology (Parts II. and III.); the second is the collection of original contributions (Part I.). Are these two departments equally well carried out? We think not. The 'Jahresbericht' is conceived and carried out on a magnificent scale. To compare favorably with it the original portion should consist of some of the very best work of the best French writers on psychology. Without wishing to cast a shade of disparagement upon the writers who have contributed to the volume, we are forced to say that the contents fall considerably below this standard. Aside from the general reviews—which are only 'original' in a limited sense—but two or three of the papers are complete or of permanent separate value. As a rule, they are rather studies, very good in their place, but scarcely in keeping with the broad purposes of the *Année*. If the writers would contribute their *best work* it might be well to retain this feature, but as matters stand at present it would seem wiser either to dispense with it or else to transform its character completely.

Another side of the same question appears when we come to the Studies of the Paris Laboratory. Does the *Année* aim to be the organ of that institution? If so, it ought, we think, to gather in a larger proportion of the Studies that are at present scattered about in various periodicals. If not, why fill its pages with material of an obviously fragmentary character? At present the *Année* is neither fish nor fowl—or better, it may be likened to a splendid fowl, hampered and made less beautiful by the presence of a fish's tail!

If we may venture a word of advice, then, it is as follows: The *Année* should be divided into two volumes, one of which, under

another name, might be made the organ of the Paris Laboratory, with other contributions if desired. The *Année* proper could then be restricted to an oversight of the year's work in the various branches of psychology, with greater latitude in the case of 'general reviews.' The introduction of the latter into the present volume is a step in the right direction; with other matter cut out, their number and scope might gradually be enlarged. These changes would give to the *Année* a unity of purpose which it now sadly lacks, and would transform it at once into an encyclopedic work of classic importance.

What has been said above has reference to the appropriateness of a certain class of writings to the *Année*, and is not intended to reflect in any way upon the value of the articles that appear in this particular number. We shall now proceed to examine the contents of the present volume.

Of foremost importance is Dr. A. Forel's paper on the methods of comparative psychology.[1] The author frankly acknowledges his scepticism regarding the value of the results obtained by direct psychological induction. The human mind differs too radically, he thinks, from that of the lower orders, to admit of carrying over to the latter with any degree of assurance the results obtained in the former. When we consider the difficulty in mankind itself of understanding the psychological constitution of individuals differing from ourselves in social grade, intellectual status, or sex, how much more reluctant should we be to assume mental analogies in the case of beings wholly different from mankind in physiological structure!

The author cites in support of his position the case of the social insects—in particular, ants. There is a marked tendency among all writers to describe the mental processes of these insects in terms of our own. But is this comparison warranted? Take the sphere of sensation, for example: the data of the various senses differ not only directly, but also indirectly; the eye gives us accurate notions of space relations, the ear furnishes us with those of time. Both of these senses are well developed in man; in insects the most highly developed sense is that of smell. In man this latter sense gives (explicitly) neither spatial nor temporal data; but in insects it is evidently capable of furnishing 'distinct and rational perceptions' of some sort (p. 44). The sense of smell must then be radically different in insects from what it is in man. Thus we meet, at the very outset, an insuperable obstacle to the direct use of induction in comparative psychology.

[1] *Un aperçu de psychologie comparée.*

Passing to the biological problem, which he believes to lie at the root of comparative psychology, M. Forel traces the phylogenetic growth of the nervous system from the original neuron. For the wave of nerve activity, whether chemical or physical in character, the author proposes the name *neurocyme*. The action of neurocyme is comparatively simple within the compass of a single neuron; but when it is called upon to pass from one neuron to another the method of transmission changes: there is now a mass of terminal fibers instead of a single line of conduction. Such an alteration in the mode of transmission, the author argues, must entail a modification in the form of activity—inhibiting it, strengthening it, or causing it to be acted upon by other waves. In this 'interneurenary action of neurocyme,' at present so incomprehensible, is contained, says the author, 'the secret of our mental mechanism' (p. 27).

Up to this point the nerve phenomena are alike for all biological species; but as we proceed further we meet with a distinction. Instinct and reason denote a fundamental antithesis in the realm of mental action. To these correspond, in the physiological sphere, two distinct modes of activity, which the author terms the *automatic* and *plastic activity of neurocyme*, respectively. These are constantly in conflict with each other, and the type of an organism depends upon which has gained the mastery in its race history. Among social insects the automatic activity is well developed, and the neural coördinations are maintained by a long heredity strictly within the same lines, so that the adaptive, or plastic, activity is crushed out. Plastic activity requires far greater complexity of structure than automatic; and hence the brain of the ant, remarkable though it must be considered, is far less wonderful in its complexity than that of a human being. The distinction between automatic and plastic activity, then, is really the key to the situation, and it is only through studying the facts connected with these physiological phenomena that we can reach a proper basis for comparative psychology.

We give M. Forel's views somewhat at length, because they seem deserving rather of attention than of criticism. It is an undoubted fact that psychology is to-day leaning for support more than ever on physiology. Whether psychologists will go so far as wholly to subordinate comparative psychology to comparative physiology, in the way he proposes, we very much doubt. At the same time there is no question but that their own inductions have been too hasty, and that considerable reconstruction of the bases of comparative psychology is necessary. The fact that the critic is a student of biology as well as a psychologist certainly lends additional weight to his conclusions.

A fitting companion-piece to Forel's article is Dr. Azoulay's review of recent theories on the mode of function of the central nervous system.[1] Those who are not familiar with the recent work in the histology of the nervous system will find here a compact *résumé* of the present status of that branch. In a few pages the writer details briefly the state of our knowledge regarding the anatomy of the neuron since the late discoveries of Ramon y Cajal and others. He then proceeds to explain the theories of nerve action which have been founded on these facts. Though fair in his exposition of all, the writer shows apparently no leaning toward any of the theories; he seems personally to prefer a modification of the older view, which held to the activity of the entire nerve—now expressed in terms of the individual neuron. The style of this article is remarkably clear, and it is easily within the grasp of those whose biological knowledge is extremely limited.

Individual, abnormal and child psychology are each represented in the *Année* by a single article. *La psychologie individuelle*, by MM. Binet and Henri, is an original contribution placed (rather inappropriately) among the general reviews. It is a plea for the wider development of anthropological tests, which have hitherto been confined almost exclusively to sensation. Citing the results of Lombroso, Galton and others, the authors conclude that the differences existing among normal individuals in the sphere of the senses 'are very feeble and insignificant compared with the differences in the higher faculties' (p. 416). In all such tests of normal individuals there are two principal objects in view: first, to compare individuals and discover what elements vary and how far; and second, to trace the relations that exist between the different faculties of each individual. Both ends can be attained by a single series of representative tests, if the same series be applied everywhere. The authors examine the series proposed by various writers, and find them all incomplete and more or less impracticable; moreover, they are not fairly representative, since all neglect too much the higher intellectual processes. The real object of these inquiries being to determine not all, but merely the most important individual differences, the writers propose a series of ten tests, from which sensation measurements are omitted entirely. They include memory, the nature of mental images, imagination, attention, understanding, suggestibility, æsthetic sensibility, moral sense, muscular power and will power, and quickness of movement and of glance. These tests are described fully in the latter part of the article.

[1] *Psychologie histologique et texture du système nerveux: les récentes théories du fonctionnement du système nerveux central.*

M. Th. Ribot's *mémoire* on abnormal and morbid character[1] is suggestive rather than complete. The author discards the historic four-fold division of temperament, and adopts the three-fold classification proposed by Seeland—into strong or positive, neutral, and weak or negative, each including some sub-types. Abnormality of character consists in the union of two or more of these in the same individual. There are three cases. The first consists in the complete transformation of the individual at some period of his life. This type approaches most nearly to the normal. Paul, Augustine, Diocletian and others are given as examples. In the second we find two opposite tendencies present at once in the same person. The third is represented by great instability of character and a rapid alternation between conflicting tendencies. This is the true pathological type.

In an article on *Fear among Children*[2], Prof. Binet gives the results of a series of questions circulated among some 100 school teachers and others. He finds five principal classes of phenomena with which fear is associated: 1. Night, solitude and mystery. 2. Loud noises. 3. Objects which inspire repugnance. 4. A possible danger exaggerated by the imagination. 5. A past experience whose recurrence is dreaded. The state of the child's health is always an important factor in determining his liability to fear; on the other hand, there appears to be no relation between fear and the degree of the child's intelligence, except in so far as a highly developed imagination is more liable to furnish objects for fear. Prof. Binet notices further the effect of heredity and ill-treatment, and alludes to the well-known fact that fear is contagious. The signs of fear begin to be manifest at the age of two or three, and increase till the ninth year, when they begin to come under control, and the emotion itself tends in normal cases to be suppressed. Some of the replies are conflicting: the proportion of children susceptible to fear is variously estimated, and M. Binet's own deduction (10%) is admittedly a mere assumption. It is scarcely within our province to speak of the author's remarks on the pedagogic treatment of fear in children, but what he says may be recommended to those interested in that subject as both timely and instructive.

Along the line of experimental psychology a number of contributions appear in the *Année*. Prof. Th. Flournoy describes a new treatment of association time. In a list of 24 words, 12 belonged to some well-defined class, while the remainder had no conceptual relation with one another. Given two such lists, the subject was asked to

[1] *Les caractères anormaux et morbides.*
[2] *La peur chez les enfants.*

read in the one case all the A's, in the other all the non-A's. The time of the latter reading was considerably longer. M. Bourdon gives a variation of an old experiment on the comparative frequency of various kinds of association, and M. Xilliez brings forward a method for calculating the influence of the ordinary serial association of numbers upon our memory of a list of figures chosen at random. Prof. Van Biervliet adds a chapter to the recently developed literature on illusions of weight.

M. Victor Henri's two articles on tactile localization may be classed together as a single monograph. In an original contribution, the author describes a series of experiments which substantiate his view that the exactness of localization on the skin is independent of the exactness of two-point discrimination. Taking a number of normal subjects, he finds that the errors of localization are large out of all proportion with the sensory circles; in many cases an impact on one finger was assigned to a closely symmetrical position on another. The paper on the Sense of Locality on the Skin (*Sur le sens du lieu de la peau*) is a review of the work along the same line from Weber down. Though its outline is somewhat influenced by the author's position, just referred to, it is in every respect typical of what a general review in a work like the *Année* should be. M. Henri reproduces tables of figures from the more important authorities, which enable us to compare the results obtained by different methods of research. At the close of the article is a bibliography of 156 titles. The author promises next year a review of the theoretical side of tactile localization.

Owing to the poverty of the data, M. Passy's review of investigations on the olfactory sense is necessarily less extensive. He takes up successively the physiology of smell, olfactometry, the properties of odors, their compounds, and the reaction time for smell, giving in each case a *résumé* of the principal results so far obtained. He neglects to furnish a bibliography of the subject; but the works actually cited are put in reference form in the footnotes. In an appendix, M. Passy sums up the results of an experimental investigation by Prof. Binet and himself on the comparative psychology of smell.

In the department of physiology, MM. Binet and Courtier contribute an article entitled *Circulation capillaire de la main*. They use the graphic method to investigate the relation of respiration, etc., to circulation. The work includes experiments on a number of problems; the tracings, many of which are given, show the changes in form of the respiration and pulse curves due to different positions of

the hand and to various physiological and mental disturbances. The writers discuss at some length the errors incident to different kinds of apparatus, and the best means of avoiding them. The study is long and exhaustive, and the authors promise further researches on several additional points necessary to render it complete. The principal conclusion reached is that 'there exist, in respect to the excitability of the vaso-motor system, important individual differences' (p. 164); these differences are too great to be attributed to the apparatus, and too constant to be due to the disturbing effect of such an experiment upon the emotions of untried subjects.

Our space will permit only a passing reference to the remaining contents. M. Henri gives a *résumé* of the well-known mathemetical methods employed in the calculation of probability and error. MM. Binet and Courtier describe an apparatus for recording the intensity of impact with one or more fingers in piano playing. M. E. Gley compares the physiology of hypnotism with the action of stimulants and narcotics, and concludes that all these effects are attributable to a paralysis of the higher centers, rather than to exhaustion of the entire nervous system.

In the analytic portion of the volume the summaries are generally limited to two or three pages. More extended notices (of ten pages or more) are allotted to Delage's book: *La structure du protoplasma*, Exner's *Entwurf*, Merkel's articles in the Philosophische Studien on *Reiz und Empfindung*, and Baldwin's book on *Mental Development*.

The general bibliography at the end of the volume is this year, by arrangement, identical with that compiled for THE PSYCHOLOGICAL REVIEW.

H. C. WARREN.

PRINCETON UNIVERSITY.

A Study in the Psychology of Religious Phenomena. Parts I. and II. JAMES H. LEUBA, Fellow in Psychology, Clark University. The American Journal of Psychology, Vol. VII. Pp. 309–385.

Mr. Leuba happily avoids the common blunder of attempting to frame a definition of religion which will cover all that the word connotes. He recognizes that it was 'in early societies a complex product made up of all the fundamental needs and aspirations of man,' many of which are now clearly differentiated and are known by their several names. The noetic impulse was one of these, but not the chief one and consequently the essence of religion survives many changes of creed. Even the belief in a supersensible world and personal immor-

tality may pass away without affecting it, for they are not of its essence. It is not based upon a theory of life of any sort, but upon one of the most universal facts of human experience: "the feeling of unwholeness, of moral imperfection, of sin, accompanied by the yearning after the peace of unity.....The reality of this subjective treasure transcends all possible belief concerning the origin and end of things, because it is the psychic correspondent of a physiological growth, and consequently can in no wise fail except with that growth. ..... It may be defined in the favorite terms of Herbert Spencer as the unification by coördination of the parts segregated by differentiation of the homogeneous."

This sense of inner discord is the fundamental postulate of the religious consciousness and its resolution into harmony is its end. In the popular religions of our day we find these truths crystallized in the doctrines of sin, conversion, justification, regeneration and reconciliation with God. But our popular religious faiths are declining to their fall, and these are facts of the inner life; it is then high time for psychology 'to accept the succession which falls to it by right.' If it would, "a new creed would be born; the wings of youth would no longer be clipped in the spring of life by a scholastic dogmatism and the soul midwifery now extensively but ignorantly practised by our revivalists and pastors could be based upon a positive knowledge of the psychology of regeneration.' This, Mr. Leuba thinks, 'is the sure conquest of a near future.' May it come within the days of the present writer's life! He would journey many miles to see 'soul-midwifery' practiced in the well appointed laboratories at Worcester, and souls regenerated in accordance with the sound principles of the new psychology, without reference to the conceptions of God and immortality!

Mr. Leuba's hope that the psychologist will assume the function of the spiritual teacher will seem, as he admits, 'a fantastic dream' to many besides myself, but that does not affect the solid worth of his inquiry into the phenomena of conversion. He has collected and published in an appendix seventeen new cases at first hand, and has searched religious literature for others. Upon this material he bases his analysis: the leading stages of conversion are conviction of sin, self surrender, faith, joy and appearance of newness, especially in external nature. The phenomena are nearly constant in all ages and countries and among men of all creeds. The Christian doctrines of Justification, Faith, Grace and Depravity are attempts to formulate some intelligible theory of the basal facts of the religious conscious-

ness. The most interesting outcome of this analysis is the exhibition of the passive attitude of the convert. The guerilla warfare which his fundamental instincts and tendencies constantly wage with one another has developed into a formal battle between two groups for final supremacy over his life, while his will is in abeyance and his accredited beliefs stand in the background.

Mr. Leuba's analysis stops short at a most interesting point. One wishes to see these facts brought under more general conceptions and that he has promised to do in Part III. of his monograph, shortly to be published. It will include 'a genetic theory of sin, of moral resistance, of consent, of self-surrender,' and will especially endeavor to bring to view their possible physiological correlates.

WILLIAM ROMAINE NEWBOLD.

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A New Factor in Evolution. J. MARK BALDWIN. American Naturalist, XXX., 441–457, 536–554, June and July, 1896.

Professor Baldwin has here summarized, enlarged and unified several of his recent papers, especially those printed in *Science* (August 23, 1895, March 20, April 10, 1896). It appears that when Professor Lloyd Morgan was in America last winter he, Professor Baldwin and Professor Osborn found that they had independently reached somewhat similar conclusions regarding certain relations of ontogeny and phylogeny or to use Huxley's distinction and avoid technical words—between *development* (of the individual) and *evolution* (of the animal series). As we all know, the biological questions most eagerly discussed at present are those concerned with the inheritance of acquired characters and the causes of the variations which have resulted in evolution. Professor Baldwin has approached the problem from a new standpoint, and has, I think, formulated ideas which have hitherto had somewhat shadowy contours.

An individual can adapt itself to new conditions. For example, a carnivorous animal, such as a dog, can live on cereals. It learns new habits, and certain adaptations take place in its digestive mechanism. Now if flesh were permanently withheld from the race of dogs those individuals would get on best whose congenital variations fitted them to live on cereals, and these variations being hereditary we might get ultimately a race of graniverous dogs. It would look as though the effect of use in the individual had been inherited, whereas this need

not really be the case. We only have individual adaptations preceding in time race adaptations. It is thus possible that many of the cases quoted by Neo-Lamarckians as examples of use-inheritance are invalid.

Professor Baldwin applies this principle, which he calls 'organic selection,' especially to adaptations in which consciousness is concerned. If conscious guidance can produce useful adaptations in the individual organism, these adaptations may become hereditary in the manner described above, and we have the course of evolution directed by consciousness, but without the need of assuming the hereditary transmission of acquired characters.

The clear statement of the fact that new traits may appear first as individual adaptations, and later through the occurrence of suitable congenital variations as hereditary modifications, is important. I am not quite sure how far it may be found in earlier writers. Darwin holds that the taste of the female, an individual trait, modifies organic evolution, and it is the essence of natural selection that under changed environment those individuals will survive who can best adapt themselves to it. If organic selection is itself a congenital variation, as Professor Baldwin indicates, we are still in the *status quo* of chance variations and natural selection. We have not found 'a new factor in evolution,' still less as Professor Osborn claims (cf. *Science* April 3, 1896) 'a mode of evolution requiring neither natural selection nor the inheritance of acquired characters.' We remain ignorant as to why the individual makes suitable adaptations, why congenital variations occur in the line of evolution and why they are hereditary.

Professor Baldwin's paper is by no means confined to this one point, 'organic selection,' 'social heredity,' 'circular reactions,' etc., are commingled in a manner that will prove confusing to many readers. Indeed, I venture to say that I find the author's vigorous thinking too often obscure to an unfortunate degree. For example, I am not sure whether or not Professor Baldwin claims in this paper that the principle of 'organic selection' is set forth in his book on *Mental Development*, nor does my memory after a careful reading of the book enable me to decide the question. Or to take a more serious problem, I do not understand whether or not Professor Baldwin wishes to use consciousness—pleasure, pain, intelligence, etc.—as a *vera causa* in individual adaptations. The average reader will take it for granted that he does, and I admit that it seems to me that he runs with the hare and hunts with the hounds.

J. McKeen Cattell.

Columbia University.

VISION.

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Strosboscopie rétinienne. id. 10 Fév.
Irradiation ondulatoire de l'impression luminense. id. 17 Fév.

M. Charpentier pointed out, some five years ago, that the starting up of a light-process in the retina is followed at a very brief interval by a process of the opposite character, that is, by something which causes an instantaneous sensation of extreme blackness. Under favorable conditions there are several alternations, less striking than the first one, of light and dark, before the continuous sensation of a bright surface establishes itself. So marked is the first sensation of blackness that it has been named the *black band*, when it is formed just after the advancing border of a white sector upon a rotating wheel. The whole penomenon is referred to as *retinal oscillations*. In the series of papers in the *Comptes Rendus* whose titles are given above, M. Charpentier discusses both this subject and the 'recurrent image.' The latter was first noticed by C. A. Young, and has been best studied by Shelford Bidwell. According to Charpentier, it is not well named; it is not properly called an image, for it does not always reproduce the shape and size of the luminous object which it follows; if that is feeble in intensity, its ghost is smaller; if that is very bright, the ghost may be five or six times as large as its original. Moreover, what has been seen by many observers as a definite recurrent image is in fact merely a maximum phase of a sensation which, in its waxing and waning, lasts for a considerable time. But as M. Charpentier does not propose another name, we shall continue to use the one which has become somewhat familiar.

His method for producing the phenomenon consists in a rotating black disc with a window in it which constitutes the moving luminous object. This window is lighted up by a piece of ground glass which is itself illuminated by rays which have passed through a plano-convex lens and which come from a source of light the intensity of which can be regulated. Thus the rapidity of the moving object, its intensity and its size can all be varied at pleasure. When these conditions are all happily chosen (the author does not state what they should be, except that a single revolution of the disc should take place in from one to three seconds) the bright object is followed first by a very black in-

terval and then by a re-vivescence of itself, usually of no definite color, but bluish when the preceding light is very feeble, and of a greenish-yellow color after blue. Some have seen it only after blue, in which case it is in fact always most distinct. Charpentier finds it after all colors; others have failed to find it after red, but Charpentier used red glass and Shelford Bidwell succeeded, with an ingenious arrangement, in using spectral light. Red glass, while it is red in a very different sense from that in which any other colored glass is of its color, is usually rather more yellowish than the extreme limit of the spectrum. The black interval may have a duration of one-fourth of a second if it follows a feeble and short excitation; otherwise it lasts one thirty-sixth of a second. Here also an oscillation may be seen under favorable circumstances. These oscillations in sensation at the beginning and at the end of an excitation by light are very suggestive of the oscillations in the direction of the electrical current which are the objective effect of the action of light upon the retina.

There is an additional observation upon the black band to the effect that it may still be detected when the preliminary excitation is so short that there is no white surface for it to appear upon; it may then be seen as a band of extreme blackness *upon the recurrent image*. This would seem to be equivalent to saying that the negative reaction of the shock of the impinging light is so strong as to mask the negative reaction of the shock caused by sudden darkness. To show this it is only necessary to make a very narrow window in the revolving disk; one degree is a convenient width.

Since there is a negative reaction at the beginning and at the end of an excitation, it seemed possible that a sudden change of intensity would produce the same effect, and this was found to be the case.

The black band had been found to propagate itself beyond the place on the retina which had been effected by the original excitation, in two directions and with a definite velocity, which had been calculated. With the arrangement just described it is very easy to exhibit this phenomenon; the persistent image has attached to it a larger or smaller luminous zone of diffuse light, and (after a very short preliminary excitation) two black streamers may be seen upon this, one proceeding towards the fixation point and the other in the opposite direction, the latter resembling the tail of a comet, with its convexity turned in the direction of the movement, the other being perhaps slightly concave in the same direction. They both begin to appear at the same moment with the black band, and on either side of it; they consist, therefore, of a propagation of this negative reaction in a definite direc-

tion and with a definite velocity, a velocity of about 77 mm. a second upon the retina. It is believed that these streamers also exhibit oscillations.

By a stroboscopic method, the oscillations are found to take place at the constant rate of 36 or 37 a second, for a mean intensity of the illumination; if the intensity is much greater or much less, the rate may be from 40 to 34 per second. Another circumstance brought out is that the diffuse spot surrounding the recurrent image changes its shape, becoming sometimes more circular and sometimes more elliptical, and that this change of shape has also a rhythm corresponding to that of the successive black bands. The subject is extremely interesting. It is to be hoped that these new observations will be confirmed and extended.

C. L. FRANKLIN.

BALTIMORE, MD.

Light Intensity and Depth Perception. T. R. ROBINSON. Am. Jour. of Psych., VII., 518–532. 1895.

Admit to one eye less light than is admitted to the other; then

I. a: If but little light is excluded from the second eye, to close that eye will darken the total field, to increase its light will brighten it;

b: If more light is initially excluded, an 'indifference point' (referred to below as limit A) is reached where increase or decrease of the light admitted to the second eye produces no effect on the brightness of the combined field (Robinson, in a previous article);

c: Starting with the initial proportion below the indifference point, to close the second eye produces the same effect as to increase its light; *i. e.*, a decrease of intensity of physical stimulus results in an increase of intensity of sensation (Fechner's paradox). This effect increases from the indifference point downward, until at a certain degree of obscuration of the second eye occurs the maximum darkening of the common visual field, hence a maximum brightening upon closing it or increasing its light (Aubert's minimum point; referred to below as limit B).

II. a: If the eyes are directed by lenses to separate fields, upon which are drawn figures for stereoscopic combination, complete stereoscopic combination occurs down to a certain degree of obscuration of the second eye (called limit C below);

b: With greater obscuration, the combination is only partial, or confused, down to a certain second limit (limit D);

c: Below this second limit no stereoscopic combination occurs, but only a binocular combination of the two fields, where the objects

combine as a single surface, the lines of each being distinct, with no depth effect perceptible.

Robinson points out the facts under II., and attempts to determine, for different intensities of the total light admitted to the free eye, the proportion of this which must be admitted to the second eye at limits A, C and D; the relation of C and D to A and B; and the causes of the results found.

He establishes the following facts: The amount of light required for the second eye to produce the stereoscopic effect is, especially with high intensities, very small, and it varies with the absolute intensity. There is a considerable range between the lowest point where the objects combine (limit D), and the point where the complete stereoscopic effect is obtained (limit C). At high intensities, C is $\frac{1}{100}$ or less, D is too small for measurement; at lowest intensities, C is about $\frac{1}{2}$, D is $\frac{1}{5}$ to $\frac{1}{3}$.

The amount of light for the second eye inefficient for the total brightness (limit A) corresponds to the amount required for the stereoscopic effect (C) only at a very low intensities; at higher intensities it is much greater. It varies with the intensity and the observer from $\frac{1}{3}$ to $\frac{2}{3}$.

The minimum point of Aubert (established by him as .122 of the full light) corresponds to the limit D only at lowest intensities. The coincidence here may be accidental, or it may be that Aubert's measurement of B cannot be relied on as applicable for all intensities, and that the apparent non-coincidence at higher intensities may be thus explained.

To account for these facts, Robinson supposes an intimate coöperation of the two retinas, such that where one retina is not stimulated sufficiently to enable it to play its part in bringing about the binocular combination its energy may be supplemented by that of the other. Then the greater the amount of light admitted to the free eye, the greater will be the energy which can be spared by it to supplement that of the partially darkened eye, and consequently the smaller the proportion of light required in the second eye for the binocular combination. For complete stereoscopic combination, however, the free eye cannot aid the other, hence much more light must be admitted to the second eye to produce stereoscopic than to produce binocular combination. When part of the energy is subtracted from the free eye to aid in the binocular combination the common visual field is darkened and the paradox produced; and this is true, both at low intensities, where the free eye cannot give enough energy to produce the complete

stereoscopic effect, and where A and C coincide, and also at higher intensities, where complete stereoscopic effect is produced while yet the paradox remains, C being much below A, because the free eye still supplies some of the energy for the binocular combination, and the common field is darkened.

BROWN UNIVERSITY. E. B. DELABARRE.

LOCALIZATION OF TOUCH.

Ueber Raumwahrnehmungen im Gebiete des Tastsinnes. CHARLES HUBBARD JUDD. Philos. Studien. XII Band, 3 Heft, p. 409.

These experiments investigate our threshold judgments of the separateness of points, of direction and of continuous lines. They comprise four series. In the first a pointed bone needle (diameter not given) was set upon the skin for three seconds, then raised and placed upon the same or a neighboring point. The results show the minimal distance at which correct judgments were given of the *direction* of the second from the first point—*i. e.*, whether up, down, left or right. The second series was like the first, save that two needles were used; the first was applied, then after three seconds the other needle was applied to a neighboring point without the first being removed from its place. In the third series tests were made with lines, from 1 to 50 mm. long, cut from thin cardboard and set on the skin in four directions—vertical, horizontal and the two diagonals at 45°—the subject *to say if he felt a line or a point.* For the fourth series solid card-edges like the above were used, together with others from which the card was cut away so as to give two end points (1 mm. long), thus leaving an 'empty' distance to be compared with the 'filled' distances of the other cards; the subject *to say if he felt a point* (below threshold for twoness), *a line, or two points.*

The results of the fourth series, when compared with each other, show: (1) That the threshold distance for judging the direction of two points is less when the needles are applied successively (0.70 cm.) than when simultaneously (2.64 cm.) (2) Unfortunately the distance is not given at which the points appeared merely separate; yet it is declared—apparently a pure assumption, although probably correct—that the threshold for separateness is also less for successive than for simultaneous applications. (3) The thresholds obtained by the second series were greater than those by the first, but less than those for wholly simultaneous application. (4) The threshold distance for judging a line not to be a point (0.88 cm.) is greater than that for 'direction'

by successive stimulation, and less than 'direction' by simultaneous points. (5) Threshold for 'lines' is same for vertical as for horizontal, but greater for diagonals than for either. Only one locality was investigated throughout the four series—the volar side of arm between wrist and elbow. Five subjects. Method, that of Minimal Change.

Theoretically the author claims to show that Weber made a fundamental error in not distinguishing between judgments of 'Distanz' and of 'Grösse'. Precisely what is meant by the last term is not made very plain, but perhaps the two could be translated as judgments of 'twoness' and of 'extension,' or of 'number,' and of 'space.' He inclines to found both upon Lotze's 'local signs', but is as classically vague as Lotze himself regarding what this definitely means.

The work is not without technical faults (for example, the tests upon each person were far to few), the results can scarcely be called new, and the paper is not weighty—save in bulk. It fills 55 pages and should have been put in 5.

CAMBRIDGE, MASS. HERBERT NICHOLS.

MEMORY.

Mémoire et Reconnaissance. H. BERGSON. Revue Philosophique. XLI, 225—248; 380—399. March, April, 1896.

M. Bergson begins by distinguishing memory as habit, the organized mechanism by which we repeat, for instance, something learned by rote, and pure memory, the memory of a particular event, say the third reading. The latter he regards as an independent function, and the sharpness of this dissociation and the insistance on this independence are the characteristic features of his discussion. Memory, as ordinarily treated, is a compound, habit illumined by memory; in reality, the past is retained not only in the form of a sensori-motor habit but also in the form of particular images with all its details localized in time. In the present articles, which are part of a forthcoming work in which a full treatment is promised, an attempt is made to illustrate the independence of the memory function by showing the part it plays in the phenomena of recognition. The sense of familiarity is held, with great probability (see especially p. 241), to rest primarily, not on association of the perception with an image, but on an organized motor reaction. Where the recognition is inattentive, it completes itself in useful movements which, though inhibiting the play of the imagination, may nevertheless be accompanied by the appropriate images selected by means of the nascent movements habitually connected with them. But in active attention the recognition is completed, not by

useful movements, but by a further defining of the object. Here the intervention of images is more pronounced. M. Bergson represents the process as follows: In the first place, attention, commonly regarded as an attitude of consciousness, is, as recent discussion has shown, consciousness of a bodily attitude. The general features of the perception are determined by movements set up by the perception, its special features by images from past experience. The selection of these images is due to successive movements of imitation. The process is compared to that of a telegraph operator who controls a message by re-transmitting it. The successive attempts at analysis of the object are thus at the same time attempts at the synthesis with it of images resembling it. These movements serve as the common *cadre* both for the perception and the images. Looked at in this way, the essential element in recognitive perception is held to be a centrifugal process. It is thus—by movements from within outwards—that the images are incorporated in the perception. There are no special centres for images: what are regarded as such are merely centres for grouping sensorial impressions; but there are in the brain substance 'organs of virtual perception influenced by the intention of the reminiscence (*l'intention du souvenir*), just as at the periphery there are organs of real perception influenced by the action of the object' (397 *n*). M. Bergson devotes the whole of his second article to illustrating this theory from the perception of language and particularly from the facts of sensorial aphasia. He shows thorough familiarity with the literature of the subject and makes skilful use of it, drawing from the very sources which have been held to furnish the strongest evidence that the memory has no existence, except as a cerebral trace, independently of the act in which it is localized, precisely the opposite conclusion. The discussion contains valuable matter, and is certainly convincing in two points: the difficulty of localizing the images and the importance for the recognition of words of their motor accompaniments. Also to be commended is the attempt to be true to the dynamic quality of consciousness by exhibiting the elements in the process of recognitive perception as connected by imperceptible transitions and as supporting one another in a function; the process is compared to an electric circuit, in which all the elements are in mutual tension. On the other hand, the conception of the memory itself, influencing by its 'intention' (whatever that may mean), the centres of virtual perception, is obscure; the assumption of a sensory efferent process transmitting the images to the periphery (245) is a very questionable mode of interpreting the value of the motor side of the process; and quite unproved by any of the

facts here adduced—though something more may be expected from the volume—is the assertion (239) of the actual existence of a memory containing every detail of the past life in all the particularity of its temporal setting, a conception which strongly savors of the 'subliminal consciousness' of so-called psychical research.

SMITH COLLEGE. H. N. GARDINER.

On Muscular Memory. THEODATE L. SMITH. American Journal of Psychology, Vol. VII., pp. 453–490.

This paper is divided into two parts, to the second of which the title more particularly applies. In the first part the motor element present in the process of memorizing of nonsense series syllables is studied; in the second 'muscle memory proper, *i. e.*, memory of movements.'

In the first set of experiments series of ten syllables were presented to the subject by means of an automatic shutter, the time of exposure of each series being 20 seconds; at the close of the exposure the subject repeated aloud as many of the syllables as he could remember. The experimental conditions were kept as uniform as possible and observations were made in all the experiments of the physical and mental condition of the subject. In order to bring to light the muscular element involved in memory of this kind, the results gained where the subject was undisturbed in memorizing were compared with those gained when the subject had to repeat 'one, two, three' continuously during the experiment. The value of the memory under the two sets of conditions was tested by enumerating the number of errors committed by the subject in the process of reproducing the syllables, the errors being classified under three heads, as (1) displaced, (2) wrong, (3) forgotten syllables.

The general result was that there were more errors when the subject counted 'one, two, three' than when he read undisturbed, the increase of error varying from 12.6 per cent. with one reagent to 17.7 with another; there were no marked differences in the proportion of the three classes of error under the two different sets of conditions. That the increase of error was due to disturbance of the motor processes, which are present when memorizing is undisturbed, and not to distraction of the attention, was rendered highly probable by another set of experiments. It was found that the memory of all the five subjects was improved when they read the syllables aloud during the act of memorizing.

In the second part of the research the printed characters of the manual alphabet were employed, the characters being formed into

series of five and ten, and presented to the subjects in the way described above. In the first group of experiments the subject merely saw the characters; in the second he imitated the characters muscularly in addition; in both cases he was required at the close of the memorizing to reproduce the characters muscularly. The addition of the motor imitation was found with all the seven subjects to cause a diminution of the number of errors (classified as in the first group), the diminution varying from 10.5 per cent. to 20.7 per cent.[1] In a third group the subject was required to count 'one, two, three,' while learning visually as in the first group. The result of this modification was, in general, to diminish the errors and to make the numerical values more constant.

As the author remarks at the close of the paper, the experiments give no exact measurement of motor memory; the investigation in fact consists in the comparison of the memory in cases where the motor element is more prominent with the memory where it is less prominent. But this admission does not deprive such experiments of their value. Every attempt to give a more definite statement of the nature and extent of the motor function in mental life is to be welcomed.

SMITH COLLEGE. W. G. SMITH.

SYNOPSIA.

Entstehung und Bedeutung der Synopsien. RICHARD HENNIG. Zeitsch. f. Psychol., X., 183–222.

One will search in vain for anything new in Hennig's discussion of synopsia. The paper contains definition and classification, following closely upon the lines of Flournoy, who is frequently quoted. References to the work of other writers are very incomplete, but the illustrated descriptions of the writer's forms are given with elaborate detail which is sometimes wearisome and which seems unnecessary in the present state of our acquaintance with the subject. The most valuable part of the paper consists in the facts which it brings to bear upon the disputed question of the 'psychological origin' of synopsia. Hennig believes that many instances of 'colored hearing,' and that all 'forms' occur through personal experiences of their possessors, dating back so far in childhood that they are naturally often forgotten. A possible source of such forms is suggested by Hennig's account of his own number form which follows the line of the houses on a very irregular street of his childhood acquaintance; these houses had interested him

[1] The percentage for the subject J. P. H. should be 11.1 instead of 22.2.

chiefly through their numbers. His form reproduces not only the line of the houses, but the characteristic lights and shades of the street. Hennig also expresses very unequivocally his belief that forms are of great utility, giving at length an account of the experience of a friend who consciously refers his unusual memory for dates to elaborate mental forms.

WELLESLEY COLLEGE. MARY WHITON CALKINS.

PSYCHICAL RESEARCH.

Address by the President before the Society for Psychical Research. WILLIAM JAMES. Proc. Soc. for Psych. Research, XII., 2–10, June, 1896. Science, III., 882–888, June 19, 1896.

The Society for the Psychical Research is fortunate in its leaders. The strongest argument it can offer in behalf of the phenomena it investigates seems to me not the anecdotes and other evidence it has been able to collect, but the fact that men such as Professor James and Professor Sidgwick take an interest in these things and are partly or wholly convinced of their importance.

The presidential address of Professor James, admirably written as a matter of course, reviews the work and claims of the Society with skill and moderation. He finds that the hypnotic wave has subsided and that experimental thought transference has yielded a less abundant return than at first seemed likely. But he thinks that solid progress has been made by the report on the Census of Hallucinations and in the investigation of clairvoyance. Ghosts also should not be ignored. "Though the evidence be flimsy in spots, collectively it may nevertheless carry heavy weight." It is 'a faggot not a chain.' This, however, is an argument that can be turned both ways. When we have an enormous number of cases, and cannot find among them all a single one that is quite conclusive, the very number of cases may be interpreted as an index of the weakness of the evidence. The discovery of a great many gray crows would not prove that any crows are white, rather the more crows we examine and find to be black or gray, the less expectation have we of finding one that is white.

The 'faggot' argument, intended for the 'rigorously scientific' disbeliever, will not be so likely to affect him as the fact that Professor James has found in Mrs. Piper his 'own white crow.' This is an argument difficult to answer except by referring to the continuity of history, which, as the author says, is maintained by the Society. The ablest of men have followed alchemy and astrology, have worshiped

strange gods, have consulted witches and burned them. Geese have before now been mistaken for swans, and often to the honor of those who made the mistake. One white crow is enough, but its skin should be deposited in a museum.

COLUMBIA UNIVERSITY J. MCKEEN CATTELL.

THE EMOTIONS.

Character and the Emotions. ALEXANDER F. SHAND. Mind, N. S., V, 203–226. April, 1896.

The first part of Mr. Shand's article deals with the method and problem of Ethology, the idea of which, as a science, seems first to have been formulated by Mill, but the beginnings of which have perhaps only recently been made by psychologists in France. Ethology is regarded by Mr. Shand as a special psychological science, the statical part of which is concerned with the classification of types and circumstances and the dynamical part with the more difficult 'deduction' of types—their genesis and development and the changes produced in them by circumstances. A 'type' is broadly conceived, not merely as a dominant tendency, but as a complex of qualities possessing inner psychical connection: which connection it is the business of the science to show. And the classification of types must take account not only of the strength of tendencies, but also of the degree of their association, of the rapidity and relative persistence of the mental processes and of the intensity of the feelings of pleasure and pain connected with the emotions and sentiments: all which determinations the analysis has to render precise. The classification of circumstances, which, relative to character, are, strictly, an abstraction, must follow their 'objective and universal meaning'. This is insisted on as a principle, but no suggestions are given as to how the principle would be carried out. It may be said, however, that a complete inventory of circumstances, in the sense required by a science of types, as distinct from the biography of individuals, is not, theoretically, impossible.

The remainder of the essay is an interesting contribution to the theory and classification of the feelings. The point of cardinal importance is the distinction between the emotions and the sentiments. Emotions and sentiments differ, not primarily in respect of intensity, but in regard to growth of organization. The sentiments are highly organized habits; the emotions are relatively isolated and simple. The latter, however, tend to develop into more stable and complex feelings and to build themselves, into modifying and modified by, the sentiments.

The sentiments are substantival, the emotions adjectival; the sentiments relate to relatively permanent objects, the emotions to events. This thought is skilfully worked out, it being shown how love for an object gives rise, under varying circumstances, to a large number of emotions; how the converse effects are produced when the object is one of hatred or dislike, and how new modifications are introduced, new emotions and sentiments developed, where the object of regard is another human being or a lower animal or one's self. The classification of the feelings follows, then, the degree or character of their organization. First come the relatively unorganized feelings, including certain emotions, all the appetites and the pleasures and pains of sense. All these may, however, form one of the two subdivisions of the organized feelings, the other being the sentiments and interests. The principle for the classification of an emotion is thus its function in the sentiment. The classification of the sentiments is a more difficult matter, and its consideration is deferred. It is to be hoped that Mr. Shand, who shows fine talent for psychology of the analytic sort and writes well, intends by this to develop the essay here presented into a larger Prolegomena to Ethology. The conception of the organization of the emotions in the sentiments, the matter of special interest to the general psychologist, has in it that quality of pursuasiveness that, once grasped, it seems to have been one's own thought always.

SMITH COLLEGE. H. N. GARDINER.

EPISTEMOLOGY.

Wirklichkeitsstandpunkt. DR. RUDOLF WEINMANN. Verlag v. L. Voss, Hamburg und Leipzig. 1896. Pp. 37.

This little book, which the author calls an 'epistemological sketch,' is in three parts. Part I. is entitled 'Orientierung.' The author gives a brief statement of two fundamental ideas of Kantism which he calls 'subjective realism.' Kantism is realistic because it recognizes a world of objective reality independent of our consciousness, and which lies at the basis of our phenomenal world. It is subjective because it makes an absolute cleft between this world of 'things in themselves' and our world of phenomena. The author next affirms a realistic position. He says that out of the original psychic state, the simple 'da sind,' unfolds the world of objects independent of the subject, and also the world of ideas belonging to the *ego*. This distinction, he says, is based on the one hand, on the immediate experience or feeling of being compelled by objects in forming representations, and on the other hand on that of the mastery over objects. This, he

claims, leads to a realistic position. Realism is next contrasted with positivism and idealism, but space does not permit us to give the discussion of these points. The conclusion is that we must accept realism, and the question is as to whether or not we are to accept it in Kant's subjective form.

This leads to Part II. which is entitled 'Apriorität und Subjectivismus.' In the first section, space, time, and causality are discussed, and Kant's position stated. Then follow some arguments against Kantism. His doctrine of the 'thing in itself' is shown to involve a contradiction, since if it is a really unknown 'x' we cannot determine it negatively by denying of it space, time, and causality. The author goes on to say that the Kantian claim that the *a priori* character of these ideas involves also their subjectivity, is a mere assumption, and then he gives some arguments for the objectivity of these categories. First, the fact that we are utterly unable to represent a world without these ideas, shows that they are most real and in fact the necessity attaching to them is just as strong as that of Kant's postulates of the practical reason. Second, the fact that our knowing consciousness is in the closest relation to a part of the objective world, viz., our body, would lead us to believe that there is a harmony between the organ of knowledge and its object. Third, the doctrine of development would indicate a close relation between the subject and the object of knowledge, and shows that our psyche has arisen in dependence on the outer world, and is therefore formed to know this world. Weinmann concludes that notwithstanding all this, the *a priori* character of these categories remains untouched, only it is merely a relative *a priori*, and if our representation of the world is conditioned by our organization, this in turn is and has been conditioned by the world.

In the second section of this part the author takes up the secondary qualities, and criticizes some arguments which have been advanced for their subjectivity. It is claimed by some that we know only the effects of things on us and not the things, and that the effects have no likeness to the things. Weinmann says that the upholders of this argument have fallen into materialistic conceptions. The argument could have force only if the process in the brain was that which is given in sensation. This, however, is not the case, and the sensations which supervene upon the nervous process are to be regarded as ideal reproductions of the outer occurrence. It is no absurdity then to say that this reproduction is adequate, and in fact the burden of proof lies with him who denies this. Further, the doctrine of 'specific energies' has been urged in support of the theory of the subjectivity of our sensations.

This doctrine, however, is physiological, and proves nothing for epistemology. Arguments from the *a priori* nature of our various senses, and from physics are also discussed, but must be omitted here. The author concludes that our sensations are subjective in the sense that they refer directly to our own body and indirectly to the outer world of which they are ideal reproductions. Thus, he says, we get for our general epistemological standpoint, a 'dualistic realism,' in which we have the world in space and time, and subject to the law of causality, and on the other hand the consciousnesses which have been developed in dependence on these categories of their environment, and which, therefore, bring them as *a priori* forms for the cognition of this world.

Part III is entitled 'Wirklichkeitsstandpunkt.' In this part the author states certain advantages of his position, which constitute, he claims, an indirect argument for it. He says that we escape the doctrine of 'chance' in reference to the relation of subject and object, and also the Kantian 'thing-in-itself.' We are also in touch with common sense and physical science, while the various philosophical disciplines, such as psychology, ethics, etc., presuppose the realistic standpoint. Even idealistic metaphysics in its greatest representatives holds that the given world is real, while the doctrine of the 'thing-in-itself' baffles all interpretation. The author closes by endeavoring to set aside two methodological objections.

PRINCETON. C. W. HODGE.

Zur Psychologie der Metaphysik. RUDOLF LEHMANN. Archiv f. system. Philosophie. Bd. II., 38–70. 1896.

The *origin* of metaphysics—the reference throughout is to dogmatic metaphysics—is to be found in two impulses, one intellectual, the impulse to solve the problems left unsolved by the special sciences; the other emotional, the impulse to get rid of certain misgivings and to justify the expression of certain natural tendencies. The first is but a special form of the general cognitive impulse and takes characteristically its rise in the contradictions involved in the fundamental conceptions of the empirical sciences; the essentially æsthetic impulse to systematic totality also leads, when strong, to speculations which seek to overcome the incompleteness of their systems. The emotional impulse, however, is the more primitive and controling. The phenomena by which it is awakened are such contrasts as life and death, natural law and will, altruistic and egoistic impulses, contrasts which are connected with the deepest interests of human life and form the really

vital subjects of speculation. The *conceptions* of metaphysics are all constructed on analogies of experience. Here, too, the experiences which furnish the analogies are either intellectual or emotional. The first supply the rationalistic, the second the mystical elements. In illustration of the first, the author refers to the influence of conceptions derived from the observation of nature on the metaphysics of the Ionians, Democritus, the French materialists, Schelling (magnetism), Hartmann (biological phenomena), and Spencer; to the influence of mathematical conceptions on the Pythagoreans and Spinoza; of logical on Plato and Hegel; of psychological on Empedocles, Fichte, Schelling (theosophical period) and Schopenhauer. But of greater influence than all these scientific conceptions are religious ideas, especially the idea of a personal God which supplies the analogy for every system of metaphysical teleology. From religion also come the mystical elements, the basal root of which is the sexual instinct, a truth which Plato finely recognized in his doctrine of the philosophical Eros.

SMITH COLLEGE. H. N. GARDINER.

NEW BOOKS.

An Outline of Psychology. EDWARD BRADFORD TITCHENER, New York and London, The Macmillan Co., 1896. Pp. xiv+352.

Herbart's A B C of Sense-perception and Minor Pedagogical Work. Translated, with introduction, notes and commentary, by WILLIAM J. ECKOFF. New York, D. Appleton & Co., 1896. Pp. xv+288.

Schopenhauer's System in its Philosophical Significance. WILLIAM CALDWELL. New York, Charles Scribner's Sons, 1896. Pp. xviii+538. $3.00.

The Child, Its Spiritual Significance. HENRY KING LEWIS. London and New York, The Macmillan Co., 1896. Pp. viii+222. $2.00.

L'Education intellectuelle des le berceau. BERNARD PÉREZ. Paris, Alcan, 1896. Pp. 340. 5 fr.

Manuel pratique des méthodes d'enseignement spéciales aux enfants anormaux. HAMON DU FOUGERAY and L. COUËTOUX. Paris, Alcan, 1896. Pp. xv+288.

Recherches cliniques et therapeutiques sur l'épilepsie, l'hystérie et l'idiotie. DR. BOURNEVILLE. Paris, Alcan, 1896. Pp. lxxi+250.

NOTES.

THE third International Congress of Psychology met in Munich from the third to the seventh of August. Of the 174 papers announced in advance, the following were presented before the general sessions, in addition to an address of welcome by Prof. C. Stumpf, the President: 'Pain,' by Charles Richet; 'Criminal Responsibility,' by Franz von Liszt; 'On the Localization of the Emotions,' by Guiseppi Sergi; 'On the Association Centers of the Brain, with Anatomical Demonstrations,' by Paul Flechsig; 'The Theory of Sensation,' by Franz Brentano; 'The Psychology of Genius,' by Frederic W. H. Myers; 'A Genetic Study of Primitive Emotion,' by G. Stanley Hall; 'A New Method of Testing Mental Ability and its Application to School Children,' by Herm. Ebbinghaus; 'Individual Psychology,' by Alfred Binet; 'On Memory for Sensations,' by W. von Tschisch, 'The Conception of the Unconscious in Psychology,' by Th. Lipps. We hope to give a full account of the Congress in the November number of this REVIEW.

WE are glad to note that experimental psychology has been included by the International Bibliographical Conference in London among the fifteen leading sciences to be catalogued.

THE chair of mental philosophy and logic established sometime since in the University of Cambridge has never been filled, owing to lack of endowment. £700 annually has now been appropriated for the chair, £200 of which is due to generosity of Prof. Sidgwick, and it is expected that a professor will soon be appointed.

PROF. E. B. TITCHENER, of Cornell University, will translate into English Wundt's *Physiologische Psychologie* and in coöperation with Mr. W. B. Pillsbury Külpe's *Einleitung in der Philosophie*. Miss Julia H. Gulliver, of Rockford College, will translate Wundt's *Ethik*.

THE University of Chicago has laid the corner stones of four Biological Buildings, the cost of which is to be defrayed by the $1,000,000 given by Miss Culver to the Biological Department. The Laboratory of Psychology will be located in the Anatomical Building, which will also include the work in neurology under Professor, now Head Professor, Donaldson.

MR. G. F. STOUT, Fellow of St. John's College, Cambridge, and editor of *Mind* has been appointed to the Anderson lectureship on comparative psychology, recently founded at Aberdeen.

DR. C. v. EHRENFELS, of Munich, has been appointed assistant professor of philosophy in the University of Prague.